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THE NEW YORK ACADEMY OF SCIENCES

SECTION OF GEOLOGY AND MINERALOGY

DATING THE LATEST MOVEMENTS OF THE QUATERNARY SEA LEVEL*

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Introduction

Whatever the ultimate causes may prove to be, the data so far available suggest that coastal terraces are controlled chiefly by eustatic oscillations. Inherent problems of correlation exist owing to sharp changes of altimetry in time and lack of horizontal or vertical continuity. Carbon-14 dating offers a solution to the problem over the period of the last 45,000 years. A world-wide survey now in progress already shows promise of providing some clues to the pattern of recent eustatic changes, and correlation for the last 12,000 years is offered.

Quaternary Sea Levels

The presence of coastal terraces, raised beaches, and emerged coral reefs and similar evidence of relative sea-level change along the coastlines of the world have long presented a challenge to geological interpretation. Three theories have been offered to explain them:

(1) *Continental flexure hypotheses.* Assuming a downwarping of the continental margin, a sort of constantly revived monocline has carried the continental shelf downward and the adjacent hinterland upward (Jessen, 1943; Bourcart, 1938, 1950). The traditional isostatic balance was called in as a motive force. Variations of this theory call for oscillations of continental margin (Lewis, 1937; Umbgrove, 1946).

(2) *Eustatic oscillations of sea level.* On the assumption that the water level, but not the land, was changeable, many different forces have been considered. These forces have included: tectonoeustasy, deformation of ocean basins (Chambers, 1848; Kuenen, 1954); sedimentoeustasy, the infilling of ocean basins (Suess, 1904); glacioeustasy, climatic oscillation; the volcanic addition of primary water (Daly, 1920; Kuenen, 1954); and expansion and contraction of the whole water mass under temperature control. The first two involve constant water volume and change of basin shape; the others keep the basins constant and change the volume. Change in water load would additionally lead to warping of ocean floors (Daly, 1934, p. 153).

(3) *Changes of the earth's rate of rotation.* Under centrifugal force the earth's geoid swells at the equator. A reduction in the rate or a change

*This paper, illustrated with lantern slides, was presented at a meeting of the Section on March 3, 1958.

in the location of the polar axis would result in a drop of sea level in equatorial regions and a rise elsewhere and vice versa.

I believe there may be some truth in all of these hypotheses, but that each has limitations that prevent it from explaining all the facts.

Dating Sea-Level Changes

Studies of Quaternary marine and fluvial terraces disclose that, broadly speaking, the lowest terraces are the youngest and the highest are the oldest. However, the terraces are arranged not merely in a simple downward progression; they oscillate in irregular steps. Nevertheless, Zeuner (1952a) has recognized that, subject to some generalization of dating, the main high terraces can be arranged in order and distributed along a straight curve dropping from the Pliocene to the present.

There are, however, serious difficulties encountered in studying this geophysical trend. Glacioeustatic oscillations of sea level corresponding to extreme climatic oscillations are undeniable, and they appear to be superimposed on the long-range secular curve. Furthermore, there are local and regional tectonic (including isostatic) complications that disturb the picture over broad areas. Otherwise it would be a relatively simple matter to correlate old shore lines on a purely altimetric basis.

It may be noted that "transgression" on a low sandy coast line is generally marked by the growth of coral reefs and dune ridges, successive belts representing stillstands and maximal limits. Lagoon deposits mark areas partly or wholly protected by such barriers. "Regression," on the other hand, results in the erosion, leaching, and lithification of dune ridges, and in karst formations in emerged coral, with evaporation and desiccation in the lagoons. During slow retreats or stillstands, beach ridges, shell mounds, and chenier belts develop, and peat bogs or cypress swamps tend to form behind them (FIGURE 1).

Current Program

I am currently engaged in a world-wide approach to the late Quaternary sea-level problem. Insofar as possible, the most stable areas have been selected for study. Such areas should not be currently involved in post-Glacial isostatic rebound, nor in regions of notorious seismicity or vulcanicity. They should be so distributed as to include the Pre-Cambrian and younger coast lines and the continental and insular shores of all continents.

Wisconsin Dates

The term Wisconsin (or Würm) is employed here to denote the last of the four great glacial phases of the Pleistocene; it excludes the Recent. No radiocarbon date has yet been obtained that would indicate that, at any time during the Wisconsin, the sea level rose above its present stand.

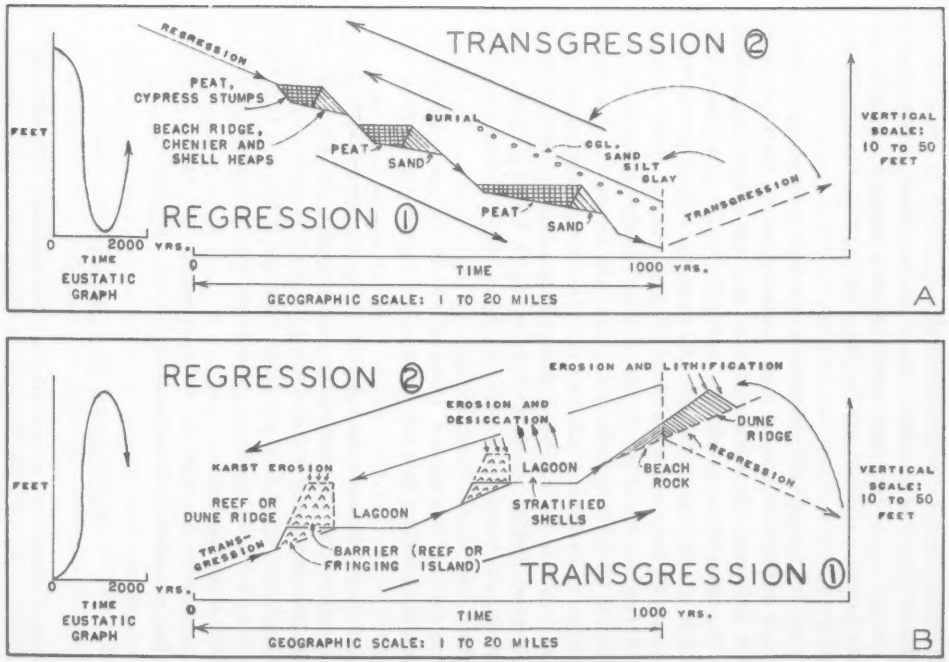

FIGURE 1. These diagrams illustrate sedimentary environments associated with (A) regression followed by transgression on a gently shelving coast, and (B) vice versa. In both, the second phase of the time curve is diagrammatically swung back to illustrate the partial erosion or burial of the previously formed deposits. One visualizes a complete cycle of about 2000 years.

Three marine terraces now seem to be recognizably the youngest of the Pleistocene sequences, and it seems likely that all three fall into the Sangamon (or Riss/Würm) interglacial. It is possible that the youngest (lowest) of these fell into an early or mid-Wisconsin (post-Iowan) interstadial that antedates the Carbon-14 range (45,000 years), but there is no convincing evidence one way or the other.

These three late Pleistocene terraces are important to the present study in several ways, not least in that there is periodic confusion between them and the younger terraces (Daly, 1920; 1934); for example see Fairbridge, 1952.

These terraces are generally referred to the former absolute elevation of sea level above the present. Storm beach deposits therefore reach somewhat higher than the stated elevation, and are related to tidal and exposure characteristics (Fairbridge and Gill, 1947). They have been named as follows: (1) the 15 to 20 m. (50 to 65 ft.) terrace known as the Main Monastir of Europe and as the Talbot of eastern North America (lowered by gentle downwarp in the Middle Atlantic States); (2) the 6 to 8 m. (20 to 25 ft.) terrace known as the Late Monastir of Europe and as the Pamlico of northeast North America (also as the Suffolk-Ingleside); and (3) the 3 to 5 m. (10 to 16 ft.) terrace known as the Epi-Monastir of Europe (Zeuner, 1956), as the Ouljian of North Africa (Gigout, 1949), and as the Dismal Swamp and Princess Anne of North America (Wentworth, 1930; Cooke, 1930; Price, 1956).

Numerous radio carbon dates of these terrace deposits all indicate that their formation was older than the range of the method would permit us to measure.

Dating of late Wisconsin marine formations is unfortunately almost entirely restricted to the area of the Mississippi delta where the unraveling of the subsidence and compaction factors is most complicated. Fisk and McFarlan (1955) have presented a picture of the late Quaternary transgression there, rising from a late Wisconsin minimum of about 450 feet below present sea level.

I now propose to consider the data for the Recent epoch, beginning about 10,000 years before the present (B.P.) or a little earlier.

The Flandrian Transgression

The classic picture of the post-Glacial transgression was obtained in northern France (and adjacent Belgium) by Dubois (1924), who identified the following levels: (1) Upper Flandrian: Dunkirk beds, about present sea level, transgressive to present; (2) Middle Flandrian: Calais beds, 0 to -15 m. (below sea level), transgressive with short regression at end; and (3) Lower Flandrian: Ostend beds, -15 to -30 m., transgressive with short regression at end.

This oscillatory sequence can be matched in many parts of the world, and a combination of pollen and radiocarbon techniques is providing the dates.

For the earlier history, the evidence is not very precise owing to paucity of dates. In the North Sea area, Nilson (1948) put the drowning of the peats of the Dogger Bank (about -40 m.) at around 8000 B.P. Terraces offshore are extraordinarily difficult to date, but reference to our graph may suggest some tentative correlations.

Youthful-looking drowned reefs off the west coast of Florida occur at 26 m. (14 fm.) and 34 m. (18 fm.) and rise from a 40-m. (22-fm.) platform (Jordan, 1952). In the northwest Gulf of Mexico, Parker and Curray (1956) describe primary platforms at 9, 31, and 45 fathoms, punctuated by steep-sided banks that may be biohermal reefs, possibly surmounting salt domes; their fossils indicate former warm intertidal conditions.

On the Marshall Islands, hard (beach-rock) bands penetrated by drilling at 50 to 80 feet seem to correspond to well-marked platforms offshore at about 15 to 21 m., or 8 to 12 fm. (Emery, Tracey, and Ladd, 1954). A deeper horizon of 160 to 168 feet corresponds to a 52-m. (28-fm.) erosional platform.

Shallow (Flandrian?) terraces were identified by the writer by aqualung exploration at -9 m. (5 fm.) and at 3 m. (1½ fm.) in the region of Fremantle-Cottesloe-Rottnest in Western Australia. It was significant that a clearly marked beach rock was found at -2 to -3 m. that contained shells and coral fragments.

It is interesting that a terrace at -2 to -3 m. (below low-tide limit) has been noted in several parts of the world and was recently recorded in Morocco by Stearns (1956); in this case the figure given was -4 m., but the datum used was mean sea level (m.s.l.) and with a tide range of 4 m., which would put the figure at 2 m. below low-tide level.

In Bermuda, a drowned cedar forest at 7 to 16 m. below sea level gave an age of 11,500 years that corresponds well with the warm-up of Two Creeks time (Alleröd stage of Europe). It is overlain by a peat of about 6900 years B.P. (Kulp *et al*, 1951).

World-wide radiocarbon figures combine to provide us with a rough model for the Flandrian transgression. They afford a striking confirmation of the picture already drawn up by Dubois (1924), deGeer (1940, 1951), and Godwin (1940, 1945) on the basis of normal stratigraphy, varve chronology, and pollen dating.

A particularly good record of the transgression is to be seen in the dates obtained from borings around San Antonio Bay on the central Texas coast by Shepard (1956). This sector appears to be moderately stable. Most of the dates are from oyster banks, and the question arises as to the former ecologic range of the oysters; a comparison with similar environments elsewhere suggests that an average depth of 3 m. would be reasonable. This figure could thus be added to each of the reported

depths, except the lowest, which is for a fluvial deposit (wood). In sequence, then, we have 9800 (27 m.), 9300 (26 m.), 8900 (19 m.), 6100 (15 m.), 5150 (9 m.), 2100 (6 m.). Since carbonate samples generally are rather liable to "enrichment" or differential loss, the dates have unusually large factors of possible error. However, they seem to fit rather well the graph of Recent levels herewith submitted (FIGURE 2). Factors of subsidence due to tectonic downwarping or compaction would appear to be slight.

For the Mississippi delta region there are many dates, although extreme complications are introduced owing to subsidence, cutting, and filling by the Mississippi and by other such factors. However, there is a series of extremely useful dates from the region of New Orleans (Fisk and McFarlan, 1955; Brannon *et. al.*, 1957; Broecker, 1957) that, when taken in comparison with those for other parts of the world, suggest a rather uniform subsidence gradient for this central part of the delta. Some allowance must be made for the former ecologic level of the dated material. Accordingly, for fossil wood a factor of plus 1 m. is subtracted from the total depth, assuming that floating wood carried downstream in flood would be buried in flood plain alluvium about 1 m. above present sea level. For marine shell (not specifically correlated with beaches), an average bay depth for oysters and clams might be taken as -3 m. (Parker, 1956), and so 3 m. is added to the total depth. These are admittedly assumptions, but it seems that a small correction factor of this sort is better than none at all.

From a trial plot it is apparent that the curve of the post-Glacial rise of sea level for the Mississippi mid-delta region is nicely matched by world figures, but indicates a subsidence since mid-Recent times of about 10 to 15 m. Naturally subsidence and compaction become progressively less up to the present time. For the Rhine delta the same figure is 5 to 6 m.

The most recent sea-level oscillations are not easy to pinpoint, owing to the difficulties inherent in determining the former depth of burial or material. At levels of 50 m. or more the correction factors have relatively little capacity for altering the general picture, but with oscillations of only 1 or 2 m., the possibility of error increases.

Dating the Recent Oscillations

Evidence of the first positive peak occurs at about 9000 B.P., after the "Yoldia" transgression of the Baltic, with sea level reaching -17 m. in stable areas and -23 m. in the mid-Mississippi delta. This is closely followed by 8500 B.P. when the pre-Boreal warm phase reversed to the short Boreal continental revival (the "pre-Neolithic oscillation" of Walton, England, according to Zeuner, 1952a, and of northwest Europe generally), marked by the Cochrane ice return of Ontario and the Bothnian

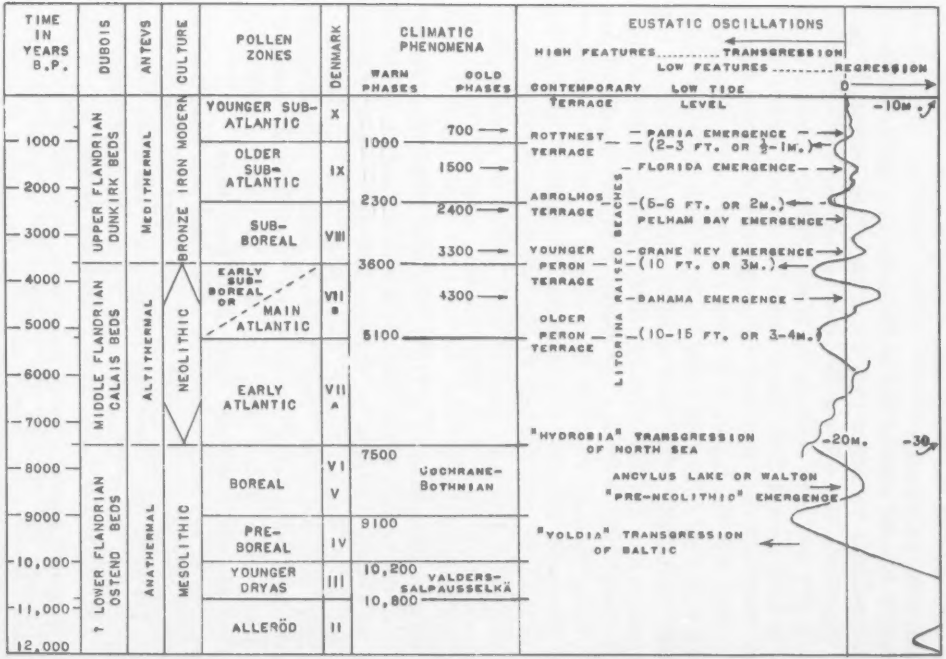

FIGURE 2. Correlation chart and graph of sea-level oscillations of the last 12,000 years based on radiocarbon dates. Note a break in the datum line on the curve from -20 m. to 0 between 7000 and 6000 B.P. Some doubts in the correlation of the European pollen zones are indicated by the diagonal between "early" sub-Boreal and "main" Atlantic.

of Scandinavia. The same oscillation is even visible in the deltaic regions where active subsidence is in progress. Dates from both the Mississippi and Rhine deltas show this feature (Shepard and Suess, 1956; van Straaten, 1954); likewise the more stable Yarra delta of Melbourne, Australia (Barendson *et al.*, 1957). The pre-Neolithic oscillation is also seen on the isostatically rising margins of Scandinavia (Rosendahl, 1956, FIGURE 2).

After this, a renewed warm-up was accompanied by a rise of sea level. This Boreal-Atlantic Transition (B.A.T.) of the pollen analysts is particularly significant, and radiocarbon dates by Godwin *et al.* (1957) fix it at 7500 B.P. There is no evidence yet of any important negative oscillation for at least the next 2500 years; only a steady warming and rise of sea level. This was the "Hydrobia" transgression of Northern Europe.

At about 5000 B.P. the sea stood at +3 or 4 m. (10 to 12 ft.) above the present as shown by dates from Point Peron, Western Australia (local description, Fairbridge, 1950), from emerged coral in Queensland and from a terrace in New Zealand (dates by Deevey, personal communication; Rubin and Suess, 1955; Ferguson and Rafter, 1954). We shall call this the Older Peron Terrace. This was the maximum of the "Altithermal."

A brief emergence followed, our "Bahama Emergence" seen in the peats of the Bahamas, New Zealand, Florida, Maine, and the Mississippi. The minimum was at 4300 B.P., and matches a known cool climatic phase (Broecker *et al.*, 1956, 1957; Kulp, 1952; Stevens, 1956; and others). Sea level dropped probably to -3 m.

A second transgression to +3 m. (10 ft.) occurred. This transgression, which we call the Younger Peron Terrace, is seen all over the world and is dated in Western Australia (at Rottnest Island), Victoria, Algeria, Alaska, and in the western Pacific. Dates range from 3900-3400 B.P. (Libby, 1952; Broecker *et al.*, 1956; Deevey, personal communication; Rubin, personal communication). Some of the "litorina raised beaches" of Scandinavia came at this time, but owing to isostatic uplift they reached their maximum at different times for different areas.

A sharp drop of sea level, marked by widespread peat formation (the "sub-Boreal" cool phase), followed. Two low peaks are provisionally identified. The first, the "Crane Key Emergence," was dated first from peats collected by R. N. Ginsburg in the Florida Keys as 3300 +/-240 B.P. (W. 149, Rubin and Suess, 1955), indicating perhaps a -2 m. sea level. A recognized glacial advance and cooling coincided at 3300 B.P.

There followed the "Pelham Bay Emergence," at about 2400-2800 B.P., a recognized cool period when sea level dropped to -3 m. and forests grew on the foreshore at Pelham Bay, N. Y. (C. 943, Libby, 1954). These "drowned forests" are rather common along the shores of eastern North America and western Europe (see Zeuner, 1952a; Zwart, 1951) and many appear to fit into this date range. This time is marked also by considerable peat development in Louisiana where, in spite of sub-

sidence, a temporary drop of sea level such as this would certainly favor peat development (Brannon *et al.*, 1957).

A sharp rise of sea level about 2300 B.P. (thus early Roman times, about 200 to 300 B.C.) introduced a third positive stand at 1½-2 m. (5-6 ft.), our "Abrolhos Terrace" after its ideal development and emerged coral reefs in that island group off the coast of Western Australia (Teichert, 1947; Fairbridge, 1948). This terrace, which is so widespread in the Pacific Ocean as to be found on every coral islet, has been attributed to contemporary storm waves by Douglas Johnson (1933) and to differential weathering by E. S. Hills (1949). This same date, also recorded by radiocarbon in New Zealand (N.Z. 49), coincides with that of the Perdue Chenier ridge of Louisiana (Brannon *et al.*, 1957). In western Europe there are widespread data to indicate a positive oscillation at this time (Zeuner, 1952a). In Italy many early Roman and Greek structures built near the shore are now farther inland; some coastal sedimentation has occurred and some crustal instability is known, but numerous examples suggest something more than a coincidence.

There followed, at about the time of Jesus of Nazareth, or 2000 B.P. an additional drop of sea level to about -3 m., marked by widespread peat formation and coastal forest growth (now "drowned"). We call this the Florida Emergence because of the excellent examples found there; dated peats of this phase are common also in New Zealand and in Louisiana (Broecker *et al.*, 1957); Brannon *et al.*, 1957). Many examples from classical Rome are recorded, indicating seaside temples, docks, and other structures built when the sea level was lower and now covered by the sea. Again the widespread distribution speaks against local oscillations. Glaciologists record 1500 B.P. as the cool maximum.

At about 1200 to 1000 B.P. the sea rose to one half to 1 m. (2 to 3 ft.) above the present, and the youngest high marine platform was cut. We call this the Rottnest Terrace (Fairbridge and Teichert, 1953) from its development on Rottnest Island, particularly on Government House Lake (a former arm of the sea now protected from wave erosion by a dune ridge). At this time the Grand Chenier of Louisiana was formed (Brannon *et al.*, 1957) and, in the classical Mediterranean regions, much drowning of coastal structures took place, including probably the celebrated pholad borings of the marble columns of the Serapis "temple" (market); some crustal instability is certainly also present in this area (the Neapolitan volcanic region).

A last important cooling came about 700 B.P. (1200 A.D.) that is well recorded in history, and numerous coastal peats formed during the accompanying drop in sea level (for example, dated in Louisiana, see Brannon *et al.*, 1957). In the Gulf of Paria, between Venezuela and Trinidad, it was determined that the marine clams were destroyed by a sudden increase of the salinity due to emergence (van Andel *et al.*, 1954) and radiocarbon brings very precise dates around 700 B.P. Accordingly I propose to call this the Paria Emergence.

Conclusion

This review represents a well-documented series of working hypotheses. A great deal more work is needed, but already we have a picture provocative of thought for historians, geologists, geophysicists, and astronomers. Additional data are now being received from various oil companies and from institutions in Holland, Denmark, England, New Zealand, and elsewhere.

Acknowledgments

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SECTION OF PHYSICS AND CHEMISTRY

THE CONFIGURATION OF FLEXIBLE POLYMER MOLECULES AT LIQUID-SOLID INTERFACES*

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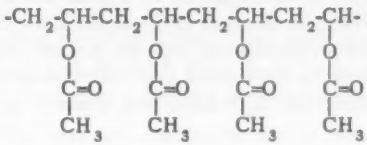
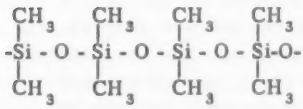
The Configuration of Flexible Polymer Molecules at Liquid-Solid Interfaces

Many macromolecules are long and chainlike in character, with the monomeric link of the chain four or five angstroms long and, roughly speaking, equal in width. The number of links to a chain may vary from 2 or 3 up to 10,000 or 20,000, though the name macromolecule is usually restricted to polymeric chains containing at least 40 or 50 units. In this report we shall restrict ourselves to those macromolecular chains that coil and intertwine in a more or less random manner both in bulk and in solution. Examples of such molecules are given below:



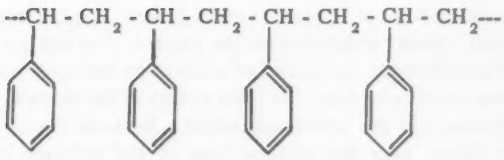
(1)

Polyvinyl acetate



(2)

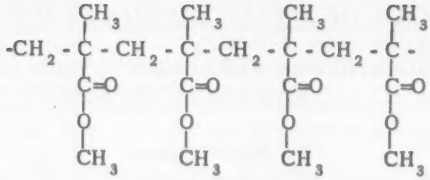
Polydimethylsiloxane



(3)

Polystyrene

*This paper, illustrated with lantern slides, was presented at a meeting of the Section on March 4, 1958.



(4)

Polymethylmethacrylate

The coiled and wormlike configuration of these materials has been established in a number of ways: from light-scattering and viscosity measurements in solution, from rubberlike electricity in the solid state, and from a variety of other corroborating information. The configuration of these molecules at an interface is modified by additional attractive and repulsive forces and, in the case of a solid surface, by the obvious steric condition that segments of the chain cannot penetrate the impermeable solid surface.

If a small molecule, for example gaseous krypton, is deposited on a surface, the analysis of the experiment is in the first approximation simple. The adsorption is accompanied by a decrease in entropy because of the loss of translational degrees of freedom. This is compensated by a gain in energy due to adsorption, and the amount adsorbed, as in all thermodynamic systems, is the amount that corresponds to a minimum free energy for the gas-adsorbed layer-adsorbent system. No internal degrees of freedom are affected by the adsorption, the area occupied by a single molecule is a constant, and no other molecular systems compete for the same region of the surface.

By way of contrast consider the problem of polymer adsorption from solution. On adsorption the polymer molecule will occupy, in general, more than one site on the surface of the adsorbent. Because of the inherent flexibility of the chain, the polymer molecule may partially uncoil or coil more tightly. Consequently the internal configuration of the adsorbed chain may be quite different from its shape in solution. In addition, adsorption from solution is a replacement reaction; the entire surface is covered by either solvent or polymer, and in general one polymer molecule replaces several solvent molecules on the surface. The energy of adsorption will be the difference in energy of adsorption between polymer segment and solvent molecule. Also, the total energy of the system is changed in the neighborhood of the adsorbing surface because the polymer concentration is higher near the surface than in the solution proper. The contribution from this effect can be either positive or negative, depending on the relative magnitudes of the polymer-polymer, polymer-solvent, and solvent-solvent interactions.

The entropy changes on adsorption are complicated. The polymer loses three degrees of translational degrees of freedom on adsorption; however,

several solvent molecules are desorbed, each of these gaining three translational degrees of freedom. In addition, the internal degrees of freedom of the polymer are inhibited since the polymer molecule is usually linked to the surface at more than one segment, the amount of restriction depending in part on the relative positions of the adsorbed segments along the polymer chain and also on their arrangement on the adsorbent. Thus there is an increase of translational entropy upon adsorption, though there is a loss of rotational entropy. The translational gain is the predominant factor, leading to an over-all increase of entropy upon adsorption for the entire system. This is a unique property of large molecules that accounts for the tendency of the polymer to adsorb beyond the extent that might be expected from its chemical structure alone. Also, the total energy of the system is changed in the neighborhood of the adsorbing surface because the polymer concentration is higher near the surface than in the solution proper. The contribution from this effect could be either positive or negative, depending on the relative magnitude of the polymer-polymer, polymer-solvent, and solvent-solvent interactions.

A considerable amount of information on the surface properties of polymeric substances has been obtained from force-area and surface potential-area measurements of polymer films at water-air and water-oil interfaces. Pioneering work by Crisp[1] and more recent contributions by Davies,[2] Miller,[3] and Schick[4] have been very useful in studying polymer molecules at very high dilution at an interface. A typical result is shown in FIGURE 1.

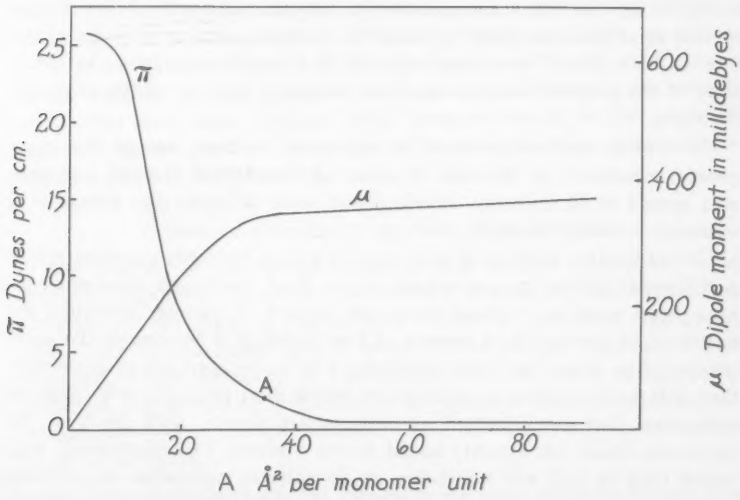


FIGURE 1. Force-area and dipole moment-area curves for polyvinyl acetate.

A thin polymer film (less than a monolayer) is spread in a trough. This film is restricted on three sides by the trough and, on the fourth side, by a thread attached to a sensitive instrument that measures the force exerted on the thread by the film. As the film is compressed, the force increases roughly in accordance with an imperfect gas law of the type

$$\pi(A-a)=nRT$$

where π is surface pressure, A is the area occupied by the film, a is the compressed monolayer area analogous to the van der Waals b, n is the number of moles of polymer, and R and T are the gas constant and absolute temperature, respectively. In the force-area curve in FIGURE 1 the force goes up slowly as the area is decreased until a point is reached when the polymer molecule can no longer lie flat in the surface. In order to compress the film further, the polar groups on the polymer molecule must be lifted out of the surface. The force increases abruptly for a relatively small decrease in area in this range. This appears at point A in FIGURE 1; further compression corresponds to a folding and coiling of portions of the polymer molecule at a distance from the interface.

The area-dipole moment curve reinforces this picture. At high dilution the dipole moment does not change with gradual compression. This dipole moment arises from the polar groups on the side chains of the polymer that are oriented with the hydrophyllic group perpendicular to the surface of the water. As the polymer film is compressed, a point is reached at which some of the molecules are pushed out of the surface, the attached polar groups are disoriented, and the dipole moment goes down. The area occupied by the film corresponding to the dropping off of the dipole moment is at the same point at which the surface pressure shows a sharp rise. In both cases these relatively abrupt changes arise from the crumpling of the polymer film that has been forced, in part, out of the air-water interface.

Force-area measurements at an oil-water interface are of the same general character as obtained at air-water boundaries. Not all polymers will spread at an air-water interface and some of these may spread if a water-oil boundary is used.

The adsorption studies of polymers on solids have been carried out in a different surface concentration range. Most published measurements have been made on systems where the amount of polymer adsorbed per unit area of surface is in excess of that found in a monomolecular compressed film at an air-water interface. The investigations of adsorption that will be discussed in what is to follow deal primarily with polymer molecules that are adsorbed at only certain points, with much of the molecular chain not directly bound to the surface. The unadsorbed segments tend to coil and intertwine as do polymer molecules in solution, subject to the obvious restraints of the immobility or relative immobility of adsorbed segments on the same chain.

The amount and character of the adsorbed polymer layer may depend on several factors: (1) chemistry of the active groups on the polymer, on the solvent, and on the adsorbent; (2) irregularity of the adsorbent surface, pore size in adsorbent, and kinetic and equilibrium effects; (3) interaction between pairs of components in the solvent-polymer-adsorbent system and polymer-solvent, polymer-surface, and solvent-surface attractive forces; (4) the effect of temperature; (5) the molecular weight and molecular weight distribution of the polymer; and (6) reversibility of adsorption.

Experimental Procedure in Polymer Adsorption

A dilute polymer solution of known concentration is shaken with a quantity of adsorbent and analyzed after adsorption. The amount of polymer adsorbed per unit of adsorbent may be calculated by difference. The result is plotted as a function of solution concentration, which yields the adsorption isotherm. In what is to follow, A is the amount of polymer adsorbed per unit of adsorbent in mg./g. at any given concentration and A_∞ is the amount of polymer adsorbed on the flat part of the isotherm.

Rate of Adsorption

In order to plot equilibrium adsorption data, preliminary measurements on the rate at which equilibrium is attained are necessary. Experiments on the adsorption of polyvinyl acetate[5] were carried out on two adsorbents, a spherical iron powder* with a smooth surface and a porous aluminum oxide adsorbent.† The results are shown in FIGURE 2 and are typical of polymer adsorption kinetics. In those cases where the surface is accessible to the bulky slowly diffusing polymer molecules, equilibrium is attained within an hour and, in some cases, within 10 or 15 minutes. If the surface is very irregular, adsorption equilibrium is not attained even after many hours because even vigorous shaking of the adsorbent-solution mixture does not speed the diffusion of the large macromolecules into the pores of the adsorbent.

General Property of the Adsorption Isotherm

The quantity of polymer adsorbed in most polymer adsorption experiments is considerably greater than can fit in a monolayer on the adsorbent surface, yet the shape of the isotherm is that of monolayer type adsorption. This is consistent with a model of the polymer molecule that is adsorbed on the surface at a small fraction of the active groups in the molecule, the remaining segments of the polymer chain penetrating into the solution phase, but still attached to the surface by adsorbed segments on the same chain.

*SF powder manufactured from iron carbonyl by the Antara Chemicals Division of the General Dyestuff Corporation, New York, N. Y.

†Activated alumina, grade F-20, manufactured by the Aluminum Company of America, New York, N. Y.

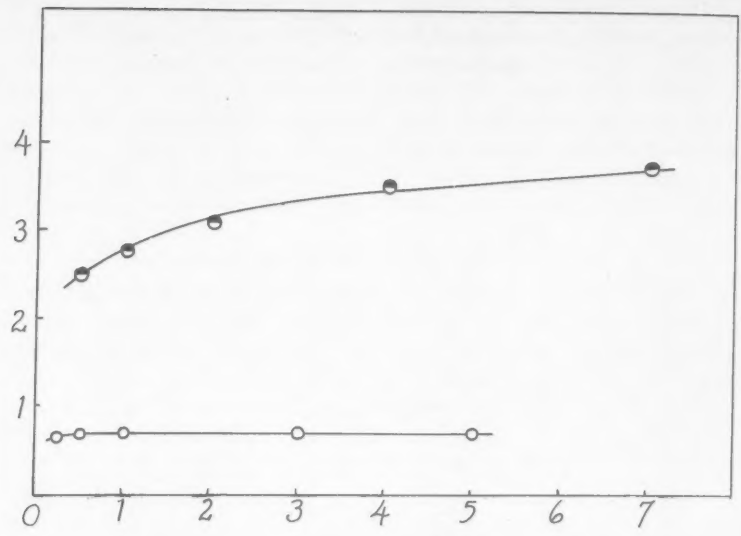


FIGURE 2. The rate of adsorption of polyvinyl acetate on a porous and nonporous adsorbent. Symbols: ⊖ = polyvinyl acetate adsorption on a porous aluminum oxide adsorbent; O = polyvinyl acetate adsorption on a compact spherical iron powder. Ordinate: A, $\frac{\text{mg. polymer adsorbed}}{\text{g. adsorbent}}$. Abscissa: t, time in hours.

Dependence of Adsorption on Molecular Weight

The amount of polymer adsorbed generally increases with molecular weight, as has been shown by Ellerstein (unpublished results) for polymethylmethacrylate (FIGURE 3), by Koral[5] for polyvinyl acetate, and by Tanaka[6a,b] for polyethylene glycol. Moreover, if the molecular weight is very low, adsorption does not take place at all. Perkel (unpublished results), who studied the polydimethylsiloxanes, found that polymers of molecular weight 600 or less did not adsorb at all under conditions where adsorption of high molecular weight polymer took place (FIGURE 4). Ellerstein (unpublished results) in corroboration of this result noted that methyl isobutyrate, a monomeric analogue of polymethylmethacrylate, did not adsorb in an experiment in which polymer adsorption took place.

In order to understand these results, it is necessary to recall, as was pointed out earlier in this paper, that a considerable gain in entropy occurs as a polymer molecule is adsorbed from solution. This gain in entropy increases with the molecular weight of the polymer and appears to be the predominant factor in the cases cited above.

Another factor becomes important if the molecular weight is sufficiently high. Because of polymer coiling, more high molecular weight material

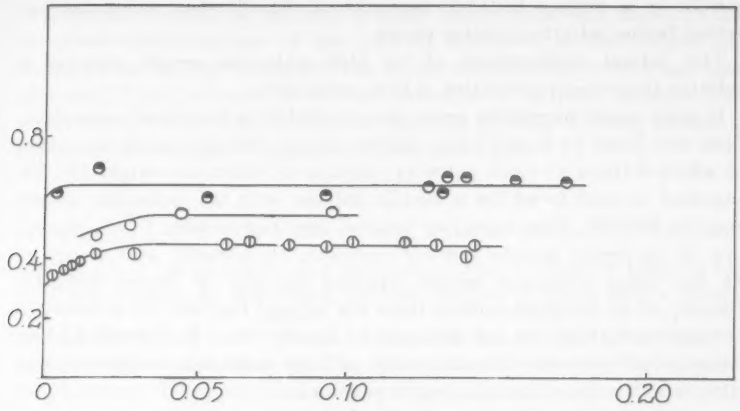


FIGURE 3. Polymethylmethacrylate adsorption on iron from benzene at 31.5° C.

Symbols: ⊖, M = 1,000,000; O, M = 70,000; ⊖, M = 18,000. Ordinate: A, $\frac{\text{mg. polymer adsorbed}}{\text{g. adsorbent}}$. Abscissa: c, concentration of polymer in g./ml. $\times 10^2$.

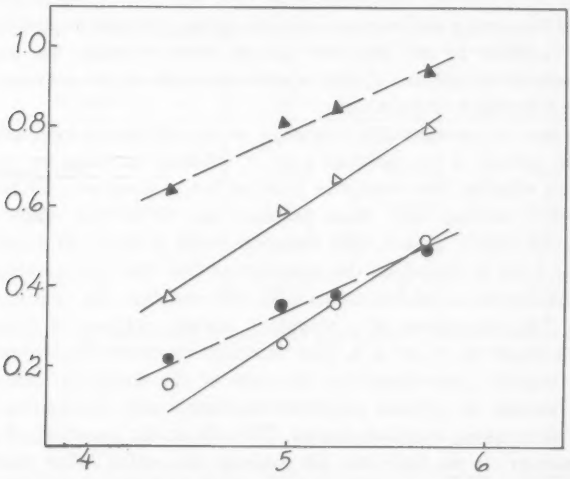


FIGURE 4. The maximum quantity of polydimethylsiloxane adsorbed as a function of molecular weight in two different solvents on two nonporous adsorbents.

Symbols: _____ _____ _____ = adsorption from n-heptane; _____ = adsorption from benzene; O, ● = adsorption on iron; and △, ▲ = adsorption on glass. Ordinate: log A_∞ $\frac{\text{mg. polymer adsorbed}}{\text{g. adsorbent}}$. Abscissa: log M_n.

can fit on a surface because segments of the adsorbed molecules can extend further into the solution phase.

The natural configuration of the high molecular weight polymer in solution is certainly indicative of this possibility.

In some cases increasing molecular weight led to decreased adsorption. This was found by Koral,[5] who used a porous aluminum oxide adsorbent on which 4 times as much polyvinyl acetate of molecular weight 275,000 adsorbed as was found for a similar polymer with the molecular weight equal to 900,000. This arose, we believe, from two factors. First, adsorption on the porous surface was not complete, but probably more nearly so for the lower molecular weight species because of higher mobility. Second, on an irregular surface there are regions that are not accessible to larger molecules, but are available to smaller ones. Both these factors conspire to decrease the adsorption of high molecular weight polymer relative to the low molecular weight polymer.

Effect of Solvent

An adsorbent in contact with a polymer solution binds to its surface either polymer segments or solvent molecules, or both. If the energy of adsorption of the solvent molecule is very much higher than the energy of adsorption of a polymer segment, the surface layer will contain no polymer at all. If this energy difference is not too great, polymer deposition takes place. It is easy to see that the solvent may determine the amount of polymer adsorbed because of the relative strength of the polymer surface and solvent-surface interactions.

If polymer is substantially adsorbed, a second factor becomes important. The radius of the polymer coil in solution is large or small, depending on whether the solvent is good or bad; a good solvent leads to a more highly swollen coil. More polymer can fit on the surface if the molecule is tightly coiled, and therefore more polymer is adsorbed per unit area. This is based on the assumption that the configuration of the adsorbed polymer is not too drastically different from its configuration in solution. The adsorption of a polyvinyl acetate polymer in several solvents[5] is shown in TABLE 1. The intrinsic viscosity ($[\eta]$) listed in the table is roughly proportional to the cube of the molecular radius. Note that the amount of polymer adsorbed increases with decreasing $[\eta]$, that is, with decreasing solution radius. The dielectric constant of acetonitrile, however, is so high that no polymer adsorption takes place. This corresponds to the extreme case of very high surface-solvent interaction.

Configuration at the Surface

One of the more exciting experiments yet to be carried out is a direct measurement of the thickness or effective thickness of the polymer layer at the solid-liquid interface. The extent of coiling and chain compression

at the surface could be obtained if these thicknesses were known. While no direct determinations of the polymer thickness at the surface are known, some indirect evidence from dilute solution viscosities has been obtained. This is based on an interpretation of polymer solution flow rates through fine capillaries. The quantity of fluid flowing through a capillary per unit time is given by Poiseuille's law

$$Q = kr^4$$

where $k = \frac{\pi p}{8\eta l}$, with p the pressure head, η the viscosity of the fluid, and l the length of the capillary. If a polymer solution deposits a layer on the capillary surface, δr units thick, the rate of flow is given by $Q' = k(r-\delta r)^4 \approx k(r^4-4r^3\delta r\text{--})Q'/Q = 1\frac{-4\delta r}{r}$. By measuring the time it takes solutions of different concentrations to flow through capillaries of different radii, the thickness of the adsorbed polymer layer may be obtained. This experiment has been carried out in several laboratories[7-9] and the results are listed in TABLE 2, together with an effective hydrodynamic diameter of the same polymer molecule in solution. From these data it would appear that the polymer coil is compressed laterally and that the long axis

TABLE 1

ADSORPTION OF A POLYVINYL ACETATE (M_w = 250,000) AT 30.4° C. ON IRON POWDER AS A FUNCTION OF SOLVENT

Solvent	$[\eta]$	ϵ	$A\infty$
Carbon tetrachloride	33.0	2.24	1.54
Benzene	93.5	2.32	0.68
1-2-Dichloroethane	110.0	10.00	0.54
Chloroform	138.0	5.48	0.35
Acetonitrile	81.0	38.80	0.00

Symbols: $[\eta]$ is the intrinsic viscosity in $cm.^3/g.$; ϵ is the dielectric constant of the solvent.

TABLE 2

	M	δr	*D*
Polystyrene in toluene[7]	500,000(M_w)	1500 Å	600 Å
Polyvinyl acetate in toluene[8]	880,000(M_η)	5000 Å	1000 Å

M_w is the weight average molecular weight; M_η is the viscosity average molecular weight; δr is the effective thickness of the polymer layer on the wall of the capillary; and *D* is the hydrodynamic diameter of the polymer molecule in solution.

perpendicular to the adsorbing surface is two to five times larger than the diameter in solution. It must be remembered that these last results are dependent on a very sensitive determination of small changes in apparent viscosity; the present interpretation must be regarded as strictly provisional.

I am indebted to F. R. Eirich, Mr. Stuart Ellerstein, Jerry Koral, and Robert Perkel, my collaborators in this work, for their assistance. The financial support of the Office of Naval Research, New York, N. Y., is gratefully acknowledged.

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THE NEW YORK ACADEMY OF SCIENCES

SECTION OF BIOLOGY

CHROMATIN SEX DETERMINATION

I. APPLICATION OF THE SEX-CHROMATIN TEST TO CONDITIONS OF SEXUAL DYSGENESIS*

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The finding by Barr and his associates that the somatic nuclei of cats revealed sexual distinctions was followed quickly by the demonstration that such differences occur in other species, including man (Moore *et al.*;[2] Marberger and Nelson;[3] Davidson and Smith[4]). Following these papers, the scientific literature has been deluged by reports on the application of the phenomenon to various conditions of sexual dysgenesis and neoplastic growths. Many of these contributions have provided basically important information, but far too many have been the products of poor techniques, uninformed reasoning, and prejudiced notions. As a consequence, the beneficial stimulus to a better understanding of sexual dysgenesis that was provided by the sex-chromatin test has been diluted and misguided by numerous ill-conceived efforts to inject poorly supported ideas into the picture. However, this state of affairs is not unusual in scientific history, since almost all new concepts have experienced an assortment of misinterpretations and misunderstandings. Those concepts that are basically sound survive the misguiding influences and become established and accepted. It is almost certain that this will be true for the sex-chromatin test.

This test actually is quite simple if the material to be examined is properly prepared and the observer takes care to instruct himself adequately in its use. Poor preparations and failure to distinguish between the sex chromatin and other nuclear material, including nucleoli, have led to bizarre and unwarranted interpretations. Preparations suitable for interpretation can be obtained by strict adherence to the methods described for tissues by Moore *et al.*[2] and Marberger *et al.*,[5] and for leukocytes by Briggs and Kupperman.[6] A method for tissues reported more recently by Klinger and Ludwig[7] appears to be equally useful. It is interesting to note that Ashley and Jones[8] have called attention to the occurrence of discrepancies between the results obtained by tissue and leukocyte methods and have pointed up the doubts experienced by others, namely, that the drumsticks seen in leukocytes are not homologous with the sex-chromatin mass. It is very likely true that the drumsticks of the

*This paper, illustrated with lantern slides, was the first of three papers presented at a meeting of the Section on March 10, 1958.

leukocytes should be regarded as a sex characteristic rather than as a representation of sex chromatin.

The chromatin mass used for interpretation of the presumed genetic sex (more properly designated as chromatin sex) is found on or very close to the nuclear membrane in somatic cells (other than leukocytes). In females it is seen frequently, in males only rarely. TABLE 1 summarizes our own findings for buccal smears and skin biopsies in individuals regarded as normal males and females.

TABLE 1

SEX-CHROMATIN TEST IN NORMAL MALES AND FEMALES

	Number of cases	*Percentage nuclei with sex-chromatin masses*
Skin biopsies (males)	102	4.5 (1 to 12)
Oral smears (males)	191	0.7 (0 to 4)
Skin biopsies (females)	84	69.2 (51 to 82)
Oral smears (females)	118	54.1 (20 to 79)

It will be noted that the frequency of occurrence of the chromatin mass is greater for both sexes in skin biopsies. The reason for this is not clear. Theoretically, the mass should occur in 100 per cent of all cells in females; in males, it should not occur at all or should be represented more consistently by a much smaller chromatin mass. Various explanations may be offered, but none are entirely satisfactory, and the brief space of this article does not warrant their discussion. Here it is sufficient to say that the experience of many careful observers appears to have established the usefulness and dependability of the sex-chromatin test as a tool for the diagnosis of genetic sex. Although this is reasonably well established from a conservative standpoint, it is proper to speak of the findings in terms of "chromatin sex" rather than genetic sex.

It has been proposed that the sex-chromatin mass is the heteropyknotic portions of the *X* chromosomes.[9] This would appear to be a reasonable explanation on theoretical grounds, since the two *X* chromosomes of the female would be expected to be more conspicuous than the *XY* chromosomes of the male. Such an explanation, however, is purely theoretical and is not entirely satisfactory, since there is no assurance that the small mass seen occasionally in male somatic cells is homologous with the mass seen very frequently in female somatic cells. On the basis of existing information, it is reasonable to suggest that the chromatin mass may represent, as has been suggested by Segal and Nelson,[10] the intermitotic allocyclic regions of an autosomal pair containing the male-

determining factors. To explain this idea, it is necessary to outline, briefly, the principles of sex determination. A full treatment of the subject is available in a recent book by Goldschmidt.[11]

It is generally known that the human female carries two *X* chromosomes, while the male carries one *X* and one *Y* chromosome. The simple and generally accepted explanation of sex determination goes no further than this. However, the complete situation is rather more complex. In addition to these sex chromosomes, sex determiners occur on a pair of autosomes (*Z* chromosomes). These determiners are, in the case of man, male sex determiners. On this basis, one may assign a sex-determining value of 1 to each *X* chromosome, the *Y* chromosome having no value. The autosomal factors each have a sex-determining value of something less than 1, or a total value of < 2. Thus, in females there is a female determining value of 2 opposed by a male determining value of < 2 while, in males, there is a female determining value of 1 and a male determining value of < 2 but greater than 1. In short, the "balance" system is such that two doses of female determiners in the *X* chromosomes are able to overcome the male determiners in the autosomes, but one dose of *X* is insufficient. If one proceeds from this basis, it is quite possible to theorize that the sex-chromatin mass seen in female nuclei represents the autosomal locus that carries the *suppressed* male determining factors. In some respects this hypothesis is more tenable than the one proposed by Barr,[9] since there is evidence to indicate that the heteropyknotic portions of chromosomes are suppressed elements. At this stage of our understanding of the subject, it is unwise to press for acceptance of any interpretation of the relationship of the "sex-chromatin" to chromosomal identities. Of more importance is the recognition of need for a great deal of additional study on the subject.

Application of the test to intersexual states in man has excited widespread interest and has served to stimulate new activity in the field of experimental embryology and in the relation of intersexual conditions to developmental, endocrine, and genetic factors. It is interesting to note that American workers are inclined, for the most part, to view the several kinds of intersexuality as having different kinds of mechanisms, or combinations of mechanisms, that is, developmental, endocrine, and genetic. On the other hand, the British and, to a lesser extent, Continental, investigators seem to insist upon a genetic basis for all defects of sexual development. A recent symposium on nuclear sex, held in London, England, and summarized by Barr,[12] illustrates this point. Almost without exception, the presentations were cast in terms of their genetic implications and of the familial nature of the various kinds of defects of sexual development. There can be no doubt of the transmission of the defects of testicular feminization and of the congenital adrenal hyperplasia, but the evidence for such transmission in the cases of

gonadal dysgenesis and of the so-called Klinefelter's syndrome is tenuous, indeed. However, such has been proposed and even extended to include transvestites (Armstrong [13]).

It is not intended here to consider the details of the various types of intersexes. These details have been provided in other publications [9,10,14,15] and are discussed elsewhere in these pages. As has been pointed out by Witschi *et al.*, [15] conditions of sex inversion in man fall into three major classes. These differ in causation, time of appearance during ontogenetic development, and the characteristics of their adult expression.

TABLE 2

SEX-CHROMATIN TEST IN CASES OF SEXUAL DYSGENESIS

No distinction is made between skin biopsies and oral smears.

Sex of rearing	*No. of Cases*	*Nuclei with sex chromatin (%)*	*Range %*	*Sex diagnosis*	
				Male	*Female*
GONADAL AGENESIS					
Female	114	2.9	0 to 14	114	
Female	22	50.7	25 to 83		22
KLINEFELTER SYNDROME					
Male	69	48.9	21 to 82		69
Male	16	1.3	0 to 3	16	
Cases in which biopsies and smears were not available. Diagnosis was made by examination of testicular biopsies.					
Male	21				21
Male	5			5	
MALE PSEUDOHERMAPHRODISM (testicular feminization syndrome)					
Female	24	4.6	0 to 10	24	
CONGENITAL ADRENAL HYPERPLASIA					
Female	67	55.3	26 to 79		67
Male	7	56.8	32 to 74		7
Male	6	2.3	0 to 4	6	

In Class 1, the defect appears to originate in dysgenesis of the primordial germ cells consequent on early damage to the ovum or blastocyst (blastophoria). In instances where the primordial germ cells disappear completely during the period preceding differentiation of the gonads and sex ducts, the gonads fail to develop, and the resulting phenotype becomes a female regardless of genetic sex. These are the cases of gonadal agenesis (or dysgenesis), or Turner's syndrome. Where the genetic sex is male, we prefer to call these individuals pseudofemales. It has been the

general experience of observers who have collected numbers of these cases that the genetic males outnumber the genetic females by about 5 to 1 (TABLE 2). In cases where the number of primordial germ cells is only substantially reduced, the medullary component of the fetal gonad is able to differentiate and a testis results, regardless of genetic sex, with consequent differentiation of male secondary sex characters. As a result, genetic females appear to be males (pseudomales). These individuals are characterized by the so-called "Klinefelter syndrome." Their gonads represent the medullary components of organs originally determined to be ovaries.[16] Failure of the cortical component to form resulted in the organization of the medullas as testes – faulty structures, it is true, but testes none the less. The term Klinefelter syndrome is a poor term, but a generally satisfactory one has not been proposed. We are inclined to prefer the term pseudomale for the genetic female members of this class, who outnumber the genetic male members about 4 to 1 (TABLE 2). The phrase female pseudohermaphrodism with gonadal dysgenesis has been suggested on as sound a basis as such designations are usually made. Bradbury and Bunge[17] object to this and offer instead ovarian dysgenesis, a term that is quite unacceptable for various reasons.

It is possible, also, to explain the majority of cases of true hermaphrodism on the same basis, that is, a reduction in the number of primordial germ cells. Most true hermaphrodites show a female sex chromatin pattern and, in these cases, it is possible that those gonads or portions of gonads that are testicular in structure have lacked an adequate number of primordial germ cells to permit formation of a cortical component.

Class 2 abnormalities are the result of a hereditary trait of some mothers. Their sons tend to be male pseudohermaphrodites and very frequently have the appearance of being women characterized by the "testicular feminization syndrome" (TABLE 2). Their daughters are normal women, but one half of them carry the gene for transmission of the defect. The gonads in affected males are testes, usually undescended; various defects of the secondary sex characters occur. In the majority of cases, these relate to varying degrees of suppression of the development of the Wolffian and Mullerian ducts, so that parts of each may be present, with suppressed development of the phallus, and variable development of the other external genitalia. At puberty, the breasts usually undergo feminine development. An explanation offered for the defect has been the formation in the mother of an antibodylike substance that counteracts the normal medullary function of the fetal testes. This results in suppression to varying degrees of the differentiation of the Wolffian ducts and external genitalia, so that the resulting phenotype often appears to be a normal female.[18,19]

In Class 3 defects, the causative factor is a defect in metabolism of the adrenal cortex (congenital adrenal hyperplasia). This defect, while

not completely understood, appears to be one in which a deficiency of "21-hydroxylase" occurs in the adrenals of fetuses which receive the defective factor.[20] The result is an enhanced production of 17-ketosteroids at the expense of 11-oxygenated corticoids. It occurs, or at least is recognized, more frequently in girls than in boys (TABLE 2) and appears to have a genetic basis, possibly involving the transmission of a multiple or quantitative allele. The defect appears only after mid-pregnancy when the Mullerian ducts have differentiated, so that afflicted females show ovaries, tubes, and uteri. The urogenital sinus and external genitalia, which complete their differentiation later, may undergo varying degrees of conversion to the male type, depending on the time of onset and severity of the metabolic defect. In extreme instances, females have been raised as boys (TABLE 2). In many of these cases modern methods of recognition and treatment have provided means for correcting the consequences of the defective adrenal function.[21]

Recently observations have begun to accumulate indicating that the female children of certain women who receive large amounts of progesterone or some related compounds during pregnancy may show variable degrees of virilization. In these female children, the syndrome is a passive one in the sense that they are victims of exogenous androgen, and they do not show the progressive features of the congenital syndrome. While the mechanism at work in the mothers of these children is not clear, it is possible that their adrenal cortical enzyme systems are defective to the extent that the provision of large amounts of certain steroids results in the production of excessive amounts of androgenic substances. It is well to note in this connection that the explanation for cases of female pseudohermaphrodism that are not progressive, that show no elevated 17-ketosteroids, and whose mothers have not been treated with progesterone or related compounds may be on a similar basis, that is, the conversion of endogenous progesterone to androgenic substances.

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II. RECENT STUDIES IN CHROMATIN SEX DETERMINATION*

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Ever since Barr and Bertram[1] demonstrated the sex differences of neurological tissue in cats, attention has been devoted to the cells of every part of the body and their morphologic minutiae that contribute to the determination of chromatin sex. Most human tissues show these cellular sex differences, but nowhere is the phenomenon more interesting than in the polymorphonuclear neutrophil leukocyte. The distinction depends on the presence or absence of an extra piece of chromatin, the drumstick (FIGURE 1), rather than upon a quantitative difference in the number of cells that show a particular concentration of chromatin.

The drumstick, first described by Davidson and Smith,[2] is a rounded hyperchromatic nuclear appendage carried on a short stem a little distance from the main body of the nucleus. It has the same significance as the nucleolar satellite of other tissues and persists throughout the life of the individual despite such contingencies as removal of the gonad or administration of the sex hormones of the opposite sex. It is not a secondary sex characteristic, as has been suggested by some workers. In no circumstances will it either appear in an individual who did not previously have any or disappear where formerly it was present.

One generalization with regard to chromatin sex may be made which is of vital importance clinically: no individual with the positive (female) chromatin sex has been found with fertile testes and no chromatin-negative (male) individual has been found with fertile ovaries. This rule may be applied to the diagnosis in certain instances of infertility and to the reproductive prognosis in intersex.[3]

Thus far, there has been no disagreement between the chromatin sex of the various tissues in one individual. The chromatin sex derived from inspection of the cells of the buccal mucosa will be the same as the chromatin sex diagnosed from skin cells. Much misinformation is prevalent in this regard. It results from incorrect reports in the literature of contradictions between the results as determined at different sites. It has been our practice to follow up these reports of discrepancies by personal communications with their authors to determine how they might have arisen. Wherever this has been done it has always been possible to resolve the difficulty. The authors have either sent us unlabeled specimens that we have correctly sexed, or else they have admitted that, subsequent to their publications, they reviewed their findings and agreed with the experience of our laboratory, namely that all the tissues in a

*This paper, illustrated with lantern slides, was the second of three papers presented at a meeting of the Section on March 10, 1958.

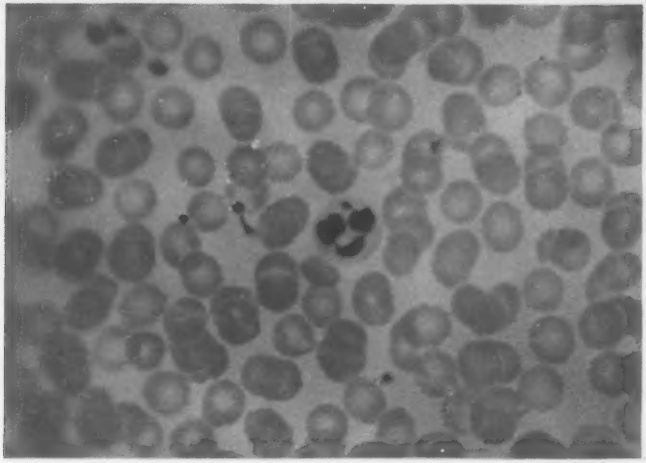

FIGURE 1. Polymorphonuclear leukocyte showing a drumstick. It is 1.5 μ in diameter. Chromatin sex is female (chromatin-positive).

given subject have the same chromatin sex. Very often the report arose from a misunderstanding of the methods and techniques of chromatin sex determination.

This consistent agreement between the tissues is reassuring to the clinician, since he is able to use whichever is convenient to him. It is also of great interest to the investigator since it seems to imply that the chromatin sex is established very early in embryological development – long before the various tissues are differentiated. If it were something acquired subsequent to conception at a particular stage of prenatal life, then it is conceivable that by now cases would have been discovered during the investigation of pathological material where the chromatin sex of one tissue was at variance with that of another tissue. It is known that events may occur during fetal growth that cause the morphologic sex to diverge from the chromatin sex and give rise to clinical paradoxes, as in the case of the woman with ovarian agenesis who is chromatin-negative and the man with Klinefelter's syndrome who is chromatin-positive, but no case has yet been observed where it could be postulated that an intrauterine change had occurred after the establishment of the chromatin sex in any one tissue. This consistency of the various tissues suggests that the chromatin sex is extremely fundamental and can probably be equated closely with genetic sex. It is a true indicator of the individual's chromatin endowment. For several years we have been searching for a case in which the chromatin sex was at variance in different tissues in one individual since the circumstances in which such a finding could be

observed would perhaps provide a key to the true implications of somatic cellular dimorphism.

Since the subject promises to yield important information, investigation should be directed to all the tissues. In those tissues that are used clinically, namely the buccal mucosa and the skin, sex diagnosis depends on a quantitative difference between the number of cells having the extra piece of chromatin. Chromatin-positive tissues show a nucleolar satellite in a large proportion of the cells, chromatin-negative tissues in only a small proportion. It has been suggested that the nucleolar satellite may be the expression of the two *X* chromosomes of female, but there is no satisfactory explanation of the significance of the same chromatin in a small number of male cells.

In the neutrophil leukocyte the situation is completely different. The drumstick, that specific appendage of nuclear chromatin that characterizes the female, has never been seen in the blood of the chromatin-negative or male individual. One drumstick is enough evidence to declare the patient chromatin-positive although, naturally, in clinical studies it is expedient to identify several of them in order to avoid mistakes arising from artifacts or from misreading some of the smaller nuclear appendages that bear no known relationship to the chromatin sex.

It is not yet possible to say which of the currently available diagnostic methods lends itself most readily to clinical studies, but it should be stated that blood is readily biopsied without surgery. The preparation of smears is so simple as to be treated at times with disdain, with bad results. The correct technique consists in carefully smearing blood on a new slide with the end of a second slide, after cleaning both in alcohol. Good preparations are obtainable by this means, and their examination permits an emphatic diagnosis of chromatin sex. At least 500 neutrophils should be examined before the patient is declared chromatin-negative. In occasional chromatin-positive subjects it is possible to examine 500 neutrophils without seeing any drumsticks. In all such cases, however, this preliminary scrutiny of 500 cells reveals 1 or 2 sessile nodules (FIGURE 2). These concentrations of chromatin undoubtedly have the same significance as the nucleolar satellite and are confined to the positive chromatin sex as strictly as the drumsticks themselves. They could be used in the diagnosis of chromatin sex but, since they lend themselves less readily than the drumsticks to exact identification, we do not diagnose the positive chromatin sex until classic drumsticks have been identified. The sessile nodules serve their purpose during the examination of the first 500 cells by prompting a more prolonged search.

A remarkable fact that has constantly presented itself during the course of our investigation has been the wide variation in the sex-chromatin endowments of different women as measured by quantitation of the neutrophil leukocytes bearing drumsticks. In some women the blood smear

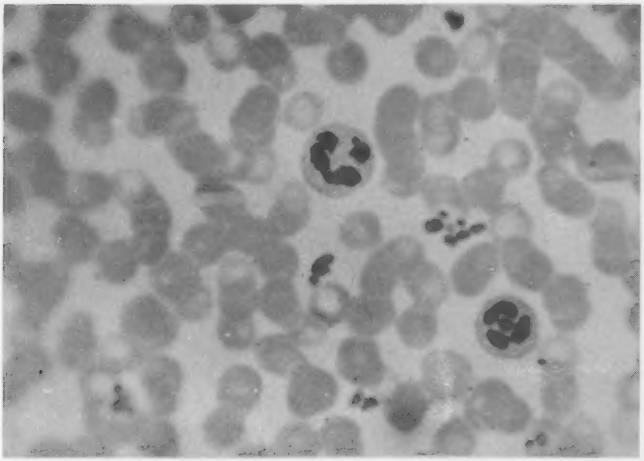

FIGURE 2. Leukocyte demonstrating a sessile nodule. It suggests that the chromatin sex is female (positive), but is not used in diagnosis.

shows drumsticks on as many as 6 per cent of the neutrophils. In others the drumsticks may be as rare as 1 or 2 per 1000 neutrophils. Although at first it appeared possible that variations in sex-chromatin endowment were related to abnormalities of endocrine function, it has now become apparent from further experience under carefully controlled conditions that there are wide individual differences between normal women. This quantity of chromatin endowment appears to be fairly constant for a given woman, but we have been unable to demonstrate any relationship between the sex chromatin endowment and any other clinically measurable aspect of individuality. Family studies are in progress and these tend to show similarities in the sex chromatin endowment of the various female members of any given family. Further statistical analysis is being conducted. No information is available to indicate whether similar differences in chromatin endowment of individuals can also be recorded from examination of other tissues, such as skin and buccal mucosa.

Other appendages project from the nucleus of the neutrophil leukocyte. They have received very little attention from hematologists and their significance is unknown, but it is reasonable to suppose that their relationship to chromatin sex and to the embryological development of the gonads and sex organs merits detailed study. It is conceivable that the small club (FIGURE 3), an appendage smaller than the drumstick, which it resembles in shape and form, is the counterpart of the nucleolar satellite seen in a minority of the cells of chromatin-negative tissues. These appendages likewise show differences in frequency between individuals, but no careful quantitation has yet been undertaken.

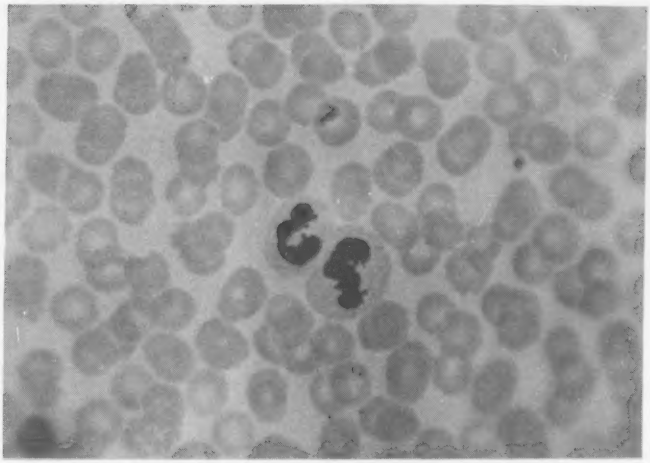

FIGURE 3. Small clubs which are similar to, but smaller than, drumsticks. These are found in blood smears of both male and female subjects.

The nature of the detailed chromatin patterns of the nuclei of neutrophil leukocytes offers a large and challenging field for research in human and animal biology.

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III. ENDOCRINOLOGIC IMPLICATIONS AND CLINICAL VALUE OF CHROMATIN SEX DETERMINATION*

By Herbert S. Kupperman

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The diagnostic importance of chromatin sex determination has been made more apparent in recent years by its application to intersex patients and those with certain endocrinopathies. Many times a study of the chromatin sex pattern has provided a definitive diagnosis where other available techniques have been less precise. The application of chromatin sex as a diagnostic tool is depicted in the endocrinopathies cited below. In patients with hypogonadism, in whom there is no evidence of any other significant endocrinopathy, the finding of a chromatin sex opposite to that of the clinical sex would be of inestimable value in arriving at a correct understanding of the problem. It is our feeling that all males with azoospermia and all females with primary amenorrhea should, in addition to the usual diagnostic techniques, be subjected to a study of their chromatin sex patterns in order to characterize their defects more adequately. The techniques of chromatin sex determination that have been employed have utilized both the peripheral blood method of Davidson and Smith [1] and the mucosal method of Nelson and his associates. [2] The reliability of both methods is discussed elsewhere in these pages. In patients with primary amenorrhea, whether of short or normal stature, in whom a negative chromatin pattern is established, it becomes almost superfluous to proceed with other diagnostic tests, inasmuch as the possession by such patients of normal functioning gonads in regard to either the hormonal or gametogenic potential is most unlikely. When the chromatin sex determination is used in conjunction with other laboratory techniques the diagnosis becomes much more apparent and readily understood. The finding of a high FSH excretion in a male or female with a chromatin pattern opposite to that of the clinical sex establishes the diagnosis of gonadal dysgenesis or agenesis without question.

The value of the chromatin sex in ascertaining the fertility potential of a patient may provide the clinician with a definite answer to a difficult problem. The finding of a chromatin pattern opposite to that of the clinical sex would make the likelihood of fertility improbable. As a result, the need for further anatomical or physiological investigation of the patient is unnecessary. The time and expense required for a more extensive

*This paper, illustrated with lantern slides, was the third of three papers presented at a meeting of the Section on March 10, 1958.

workup may thus be spared, and the patients who fall in this category may then make early provision for obtaining children other than by their own reproductive means.

Case Histories

Patient M. E. (FIGURE 1), a 28-year-old male, was evaluated because

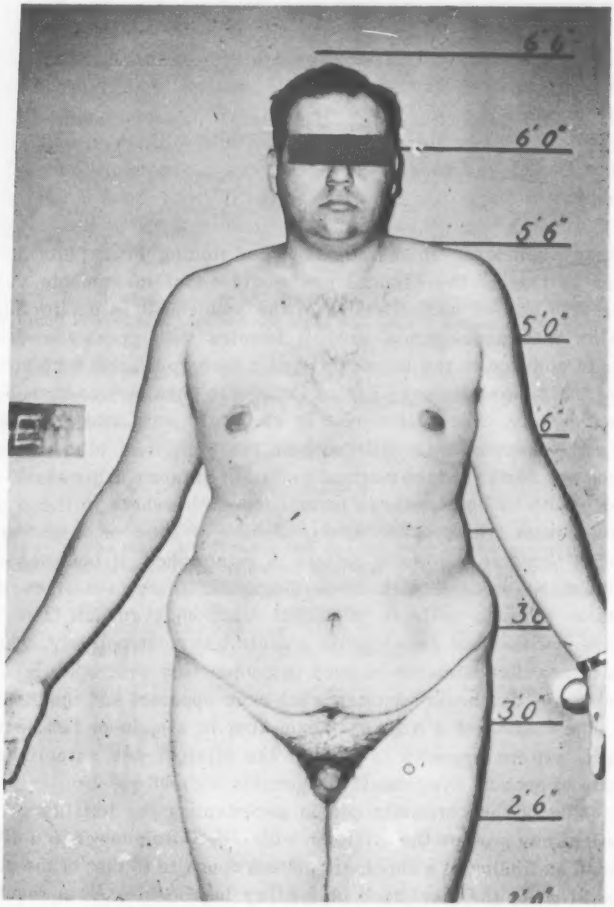

FIGURE 1. Patient M. E., a 28-year-old male with a positive chromatin pattern. Note the complete absence of any evidence of femininization. The patient, while obese, shows normal male characteristics.

of infertility and a history of azoospermia. The chromatin sex studies revealed a positive chromatin pattern. Further workup of this patient was discontinued because it was felt that there was no likelihood of his ever achieving gametogenic function capable of fertilization. Interestingly enough, the urinary gonadotrophin level in this patient did not reveal the hopelessness of the situation since it was within normal range and was not elevated.

As a rule, patients with primary amenorrhea must be considered as having a poor prognosis with respect to the initiation of normal menses and pregnancy. In some cases the study of their chromatin sex pattern may establish, more rapidly and definitely than has hitherto been possible, the unlikelihood of ever inducing functional gonadal activity. Where the chromatin pattern does not indicate a final or hopeless prognosis for fertility, the supplementation of this information with other endocrine data is necessary in order to establish a more definitive diagnosis and prognosis.

Patient J. B. (FIGURE 2), a 23-year-old Negro female, was seen because of primary amenorrhea. The chromatin pattern was positive. Urinary hormone assays revealed a gonadotrophin excretion of less than 6.6 mouse units/24 hours. The positive chromatin pattern in this patient did not contribute to the diagnosis except to indicate that gonadal dysgenesis was not present. The low FSH excretion suggested the possibility of selective pituitary gonadotrophin deficiency and, in general, would tend to rule out the possibility of ovarian agenesis. The prognosis in this patient, while poor, was not hopeless. It is interesting to note that after one course of gonadotrophin hormone and estrogen therapy the patient has now experienced spontaneous regular menstrual periods for three successive months. The gonadotrophin (pregnant mare's serum in doses of 500 I.U. 3 times a week for 3 weeks) was originally intended as a diagnostic procedure rather than a therapeutic one in order to determine the presence or absence of ovaries capable of functioning. The patient's response to the gonadotrophin, characterized by an increase in breast size and maturation of vaginal smear and subsequent menses, substantiated the impression gleaned from the gonadotrophin assay that the patient had ovaries capable of endocrine function. The initiation of ovarian function in this patient in this manner, while surprising, was certainly not entirely unanticipated, in view of the normal chromatin pattern and the low FSH excretion.

On the other hand, patient I. A. (FIGURE 3), a 32-year-old female with extremely modest secondary sex development, clitoral enlargement, and primary amenorrhea, was promptly placed in the hopeless category when the chromatin study revealed a negative chromatin pattern. The irremediable nature of her endocrine disease was substantiated by finding a high urinary FSH excretion. The diagnosis of gonadal dysgenesis was made,

FIGURE 2. Patient J. B., a 23-year-old female with a female chromatin pattern and primary amenorrhea. The patient's breast development occurred after steroid therapy. Spontaneous menstrual periods occurred after gonadotrophin hormone therapy.

thereby relegating the patient to absolute infertility and ovarian failure. Appropriate cyclic sex steroid hormone therapy has produced a pleasingly formed feminine individual capable of partaking of conjugal relationships as a result of the adequate development of her secondary sex organs and maturation of the vaginal tract. The cyclic catamenia that was induced lent further reassurance to this patient relative to her femininity.

Other clinical applications of the chromatin pattern study are in the early diagnosis of gonadal dysgenesis. Such an early diagnosis may be of advantage by indicating the eventual need for appropriate steroid hormone replacement therapy at the proper chronological age so that the asexual nature of such female patients and the grotesque eunuchoidal picture and habitus of the male counterpart may be prevented.

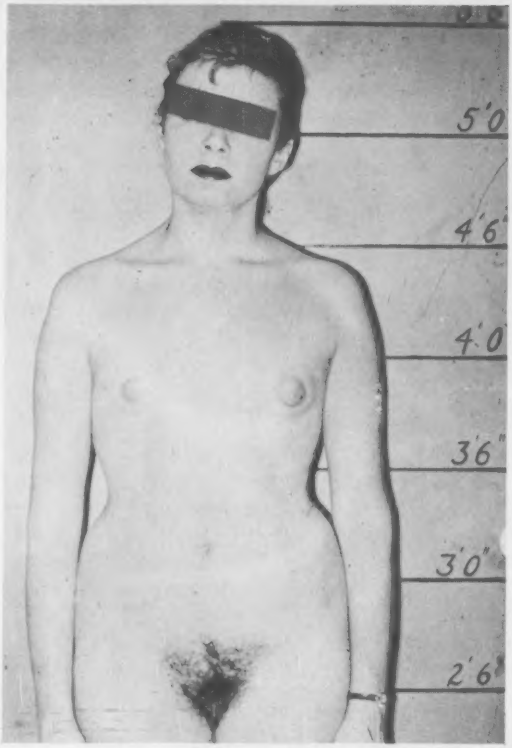

FIGURE 3. Patient I. A., a 32-year-old female with negative chromatin pattern. The patient showed no breast development when originally seen. Her present breast growth followed cyclic therapy with estrogen and progesterone.

Patient R. R. (FIGURE 4), a short and stocky immature female, age 14, with complete lack of secondary sex characteristics, was readily diagnosed as having gonadal dysgenesis by the presence of a negative chromatin pattern. Since the urinary FSH excretion in the juvenile female with gonadal dysgenesis may not yet be elevated at this time, this assay may not be of help in supporting the diagnosis. In view of the results of the chromatin studies, gonadal replacement therapy has been instituted so that when the child approaches adulthood she will not resemble the asexual habitus of patient I. A. (FIGURE 3) and thus may be spared the doubts and anxieties of one so affected.

The anatomical male with gonadal dysgenesis, as depicted in patient G. K. (FIGURE 5), age 12, where an early diagnosis was established by noting a positive chromatin pattern, makes it possible to initiate appro-

priate androgen therapy early in life. A testicular biopsy (FIGURE 6) in this patient confirmed the diagnosis without question. The early and appropriate therapeutic approach in such patients would have the advantage of preventing the occurrence of the eunuchoidal picture so characteristic of the classic Klinefelter patient. Without the use of the sex

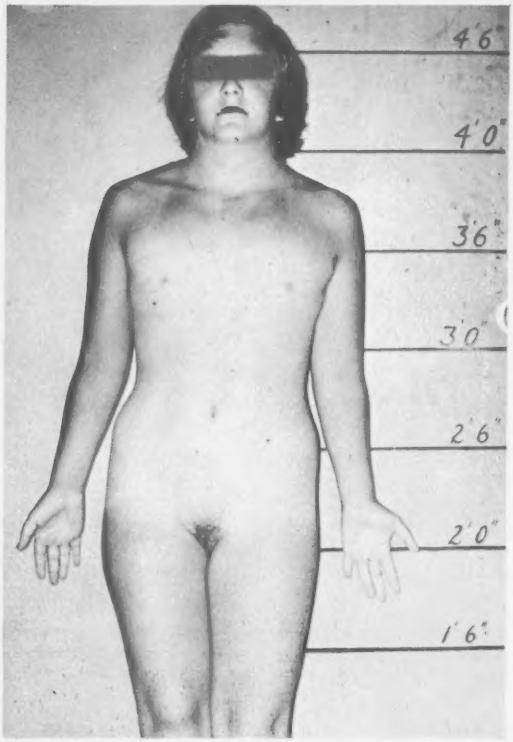

FIGURE 4. Patient R.R., a 14-year-old female with a negative chromatin pattern. Note the complete lack of any secondary sex characteristics.

chromatin technique, it is conceivable that the diagnosis in this patient might well have been missed, even though the patient showed a well-developed phallus, accompanied by small testes. The ease of performing the chromatin sex determination and of obtaining specimens for such studies makes its use readily applicable in such patients. While testicular biopsy can also establish the diagnosis, the clinician would find it necessary to proceed with extreme caution in performing or suggesting testicular biopsies in patients having the physical dichotomy listed above.

Not all patients with ovarian agenesis exhibit gonadal dysgenesis. Patient K. B. (FIGURE 7), a 15-year-old girl who had a female chromatin pattern, presents as hopeless a prognosis with respect to gametogenesis and hormonal secretion by her ovaries as patient I. A., who showed a negative chromatin pattern. In patient K. B., despite the presence of a positive chromatin pattern, the physical findings of a short female with

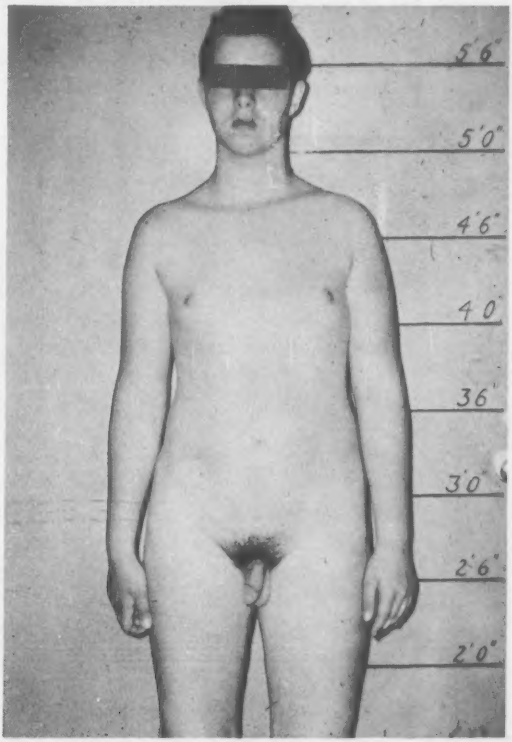

FIGURE 5. Patient G. K., a 14-year-old male with a positive chromatin pattern. Note the well-developed penis and the presence of pubic hair, not accompanied by eunuchoidism or gynecomastia.

a normal bone age, high urinary gonadotrophin excretion, and failure of gonadal stimulation to occur following pregnant mare's serum (PMS) administration, all point to the diagnosis of ovarian agenesis without dysgenesis. In this patient the supplementary endocrine evaluation and therapeutic test were paramount in establishing the diagnosis.

A third and equally important use of the chromatin technique in clinical medicine is in the intersex individual or in the patient with indifferent external genitalia. This application has attained a greater significance since it has been shown that some degree of sexual undifferentiation may be observed in infants delivered of women who have been placed upon ethynyltestosterone for the treatment of habitual abortion. Knowledge of the chromatin sex pattern may be helpful in determining the actual anatomical or gonadal sex of such a patient. Application of this knowledge plus that obtained by hormone excretion studies furnishes the physician with a double-edged sword to cut the Gordian knot of the unknown sex and thereby establish the correct anatomic sex of the child.

FIGURE 6. Testicular biopsy of patient G. K. Note the absence of spermatogenic elements and peritubular fibrosis.

A female with the adrenogenital syndrome, congenital in nature, in whom the external genitalia may appear like those of a male, can be spared the risk of neonatal death in adrenal insufficiency and a life of unhappiness and infertility if a correct diagnosis is made and if treatment is instituted during the neonatal period. Correct designation of the clinical and anatomical sex at this time will prevent the psychological

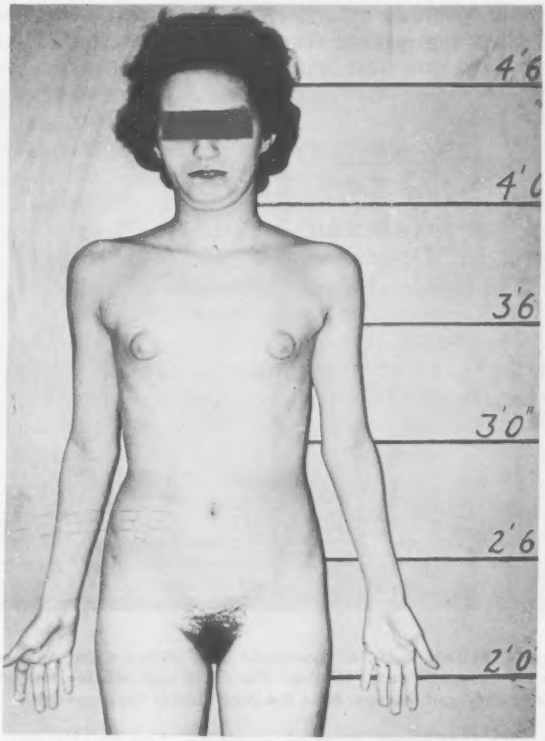

FIGURE 7. Patient K. B., a 15-3/4-year-old female with a positive chromatin pattern. Note the short stature. The minimal breast development occurred following estrogen therapy.

aberration in both parents and infants that may occur if the child is originally designated as a male and later changed to a female. An infant with a positive chromatin pattern and an elevated 17-ketosteroid and pregnanetriol excretion level, despite the gross appearance of the external genitalia as male, must be considered as a female pseudohermaphrodite due to adrenocortical hyperplasia, and the child should be brought

up accordingly. Appropriate surgical intervention will correct the abnormalities of the external genitalia, permitting any necessary abdominal surgery at a more appropriate time.

Despite the diagnostic advantages offered by a study of the chromatin sex of an infant with indeterminate genitalia, clinical judgment often must still be paramount in certain cases in determining the eventual sexual orientation of the child.

Patient M. C. (FIGURE 8), a 14-year-old male with a small testicular-like organ in the left inguinal canal and no testis palpable in the right,

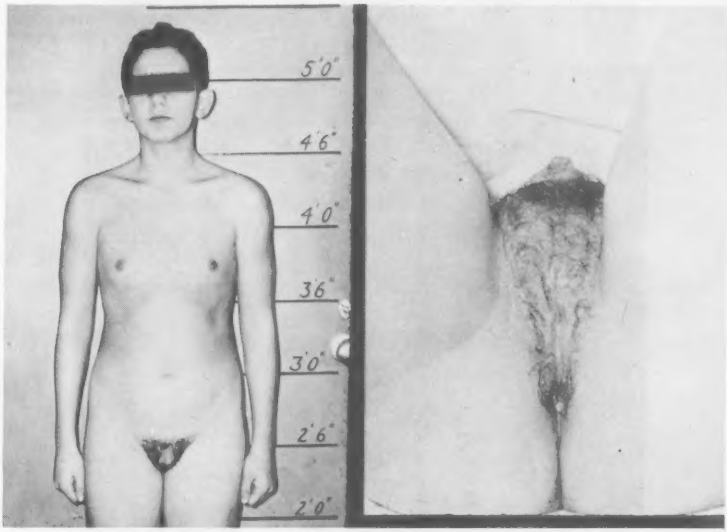

FIGURE 8. Patient M. C., a 12-year-old male with a negative chromatin pattern. Note the unusually small phallus. The pubic hair development was induced as a result of androgen therapy. Note the poor scrotal development.

was born with indifferent external genitalia. The child's sex was first designated as that of a female and later it was changed to a male, when the true nature of the genitalia became apparent. The enlarged clitoris was now considered as a penis with third degree hypospadias (the urethral opening being located at the base of the shaft of the phallus). The patient, who is a chromatin-negative individual, has been subjected to numerous plastic operative procedures resulting in a urological triumph by having the urethra successfully transverse the phallic structure. However, fistulae developed at the base of the penis so that, while the patient can project a stream during micturition, he must nevertheless still squat or sit because leakage occurs during urination at the site of the fistulae.

The boy is now a feminine-looking male with a ridiculously small phallus, denied not only the sexual activity of the male, but subject to the indignity of having to sit during micturition. The appropriate clinical procedure in this infant, despite the male chromatin pattern, would have been to rear the child, as originally planned, as a female. While the patient as a female would have been irremediably infertile, yet with surgical plastic procedures the patient would have been able to partake of conjugal relationships and function essentially as a normal female. The boy in his present status is also infertile (there has been no penile response to chorionic gonadotrophin hormone therapy, indicating evidence of testicular failure and making it unlikely that spermatogenic elements are present). Despite massive doses of androgens, there has been little phallic hypertrophy, so that not only is he sterile, but he is incapable of functioning as a male sexually. To him has fallen the unhappy lot of the neuter sex, a fate that could have been spared him by the early application of adequate clinical acumen. The child could then have remained a female and could have been brought up as such.

The chromatin sex technique offers the physician a new and valuable diagnostic implement whereby the clinical sex and prognosis for fertility of the individual may be established more firmly and easily in certain complex endocrinopathies and intersex. Knowledge so gained can confer greater wisdom of action upon the clinician with respect to appropriate management and therapy for such individuals.

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SECTION OF PSYCHOLOGY

FUTURE OF PSYCHOLOGICAL RESEARCH ON BEHAVIOR DISORDERS*

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During the last ten years I have made about four hundred visits to hospitals and clinics, serving as research consultant to psychologists working with patients suffering from behavior disorders. As an academic psychologist I have assisted in formulating and designing research projects on behavior disorders and in analyzing and interpreting the findings. *In loco* I have observed the theories and practices used in the institutional management of behavior disorders.

From this experience as research psychologist I shall try here to formulate what seem to be the most fruitful future areas of psychological research on the causation, diagnosis, and treatment of behavior disorders.

Causation

Behavior disorders usually emerge in a stress situation with which a patient has been unable to cope. We may express the relation in biological format:

$$D = f(S,G)$$

D represents a behavior disorder (variously called psychiatric disorder, mental illness, or emotional maladjustment) that has put the individual into patient status. It consists of activities perceived by competent observers as lying outside the "normal" range of behavior.

S stands for the environmental stress situation, composed of patterns of stimuli that serve to block his ongoing activity ("frustration") or to compel conflicting responses ("conflict"). Commonly, the patterns of stimuli may persist over long periods of time, that is, they may be cumulative in effect. *S* also represents disease, injury, or other physical environmentally induced impairments of the patient's smooth functioning. *G*, his genotype, refers to the inherited pattern of genes that control his behavior.[3]

Most workers accept this etiological paradigm. However, it is not helpful in any specific case, for the desire is to discover in the individual patient what particular stress-genotype patternings have produced his specific disorders. The patterns of social and physical stimuli that ceaselessly fall upon the individual are infinite in number and form; hence stresses can assume countless forms and intensities over a period

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of time. The varieties of human genotypes produced by the genetic mechanism are innumerable. In each patient the problem is to ferret out and describe the complex stress patterns to which he has been and is now subjected, and then to isolate the particular stresses that have produced deviant behavior in him an unknown genotype.

Faced with these mysteries we may understandably omit this basic problem of stress-genotype causation. We may adopt the hypothesis of intervening variables, assuming that stress situations generate in the genotype certain characteristic "inner" modes of adjusting, such as anal, or oral, or other complexes and defensive dynamisms. If we call intervening variables *I*, we thus assert the following substitution:

$$I = f(S,G)$$

which permits us to ignore *S* and *G* and express the determination of *D*, the behavior disorder, as follows:

$$D = f(I).$$

The causation problem is further simplified if we assume that in any given patient the intervening *I* states that determine his disorder have certain simple properties. We may assume, in short, that they are generalized in their effects across a diversity of situations, are not numerous, and are rather universal in form (that is, have existed in many other persons as far back as the ancient Greeks).

However, this substitution may be of one baffling problem for another. How do we know that these hypothetical *I* constructs, with their assumed special properties, exist? Demonstration of their "reality" requires carefully controlled research studies that yield positive findings. Many experiments have been conducted to prove the existence of salient, generalized, universal dynamisms of personality, but so far the results have generally been negative.

Knowledge of the more basic stress-genotype causation of disorders must come from laboratory studies. We can experimentally vary stress situations on human subjects only within a mild range; we also suffer fatal ignorance about their genetic make-up. Perforce, then, experiments on the basic causes of the behavior disorders must be performed on animals. Only from a species whose genetic make-up can be knowingly varied or held constant at will can we learn about how disordered behavior emerges. Several species, notably mice, rats, and dogs, do permit some degree of genetic control. It has already been demonstrated in mice, for example, that the incidence of seizure responses to auditory stress depends on the particular mouse genotype. The Bar Harbor program on the behavior genetics of mice and dogs provides increasing evidence of the genetic bases of emotionality, and I should not reject out-of-hand the possibility of undertaking basic studies of behavior aberrations in *Drosophila*.

From stress-genotype experiments on animals we shall develop principles of behavior disorder that should be helpful in understanding human deviance. Theories that now guide diagnosis and treatment of man but have not yet been verified by experimental evidence can be tested on animals. For example, the prevalent theory that holds that traumatization at an early age has broader consequences than traumatization at maturity can be checked. Experimental animal work on this problem has not yet provided support for the theory.[1]

A disorder that results from a *unique* relation between an individual's *G* constitution and his stress situation *S* may have profound effects in his reactions to many situations. In his classic studies on experimental neuroses in dogs, Pavlov formulated this relation as one between genetic temperament and stress conditioning. That such a formulation on animals leads to better understanding of human disorders in combat fatigue and in political, religious, and psychological conversion is vigorously urged in the recent writings of the British psychiatrist, William Sargant. An expanded program of the behavior genetics of experimental neuroses in animals is needed to support these theories.

As an example of the current emphasis on motivation analysis, the theory that environmental distortion of incentives determines behavior deviance is open to animal test. In an unpublished experiment on rats, I found the theory poorly supported. Known maze-bright and maze-dull genotypes were exposed to extreme variations in the food-incentive to encourage them to learn the maze. Satiation or deprivation produced by variation of this incentive had no effects on the performances of genetically bright or dull rats or in their power to learn (maze-errors), and only minor effects on tempo or "style" of performance (rate of running).

The belief that the cause of extreme mental deficiency or of genius is a single general organic physiological or psychological dimension is also subject to animal test. My own studies of selective breeding for maze ability in rats show that extreme deviates occupy their statuses because a large number of polygenetic factors contribute to their performances.[4] Extended to human beings, this rat paradigm means that a host of genetic and environmental factors produces extremes, and that to search for a few salient general determinants may be futile. To date, animal experiments provide no basis for a belief in a few cognitive or personality "traits." Abilities are extremely numerous; moreover, they are not generalized modes of adjusting to many situations, but are tied to specific types of situations.

Diagnosis

Current diagnostic office practice has become rather standard. The Rorschach or the Thematic Apperception Test (TAT) is used to elicit the intervening *I* variables of the patient. In each, the presenting stimuli are ambiguous figures to which the subject freely responds by verbal per-

cepts. His pattern of projected percepts, when analyzed with *expertise*, is believed to reveal his dynamics. The Minnesota Multiphasic (MMPI) is a printed list of signs and symptoms that he reads; he designates which are characteristic of himself. His profile of scores on these items is believed to describe his salient *D* symptom pattern. His intellectual status is measured by verbal and performance IQ's from a Wechsler battery, a series of scholastic-type tests and simple problems-to-solve. IQ and subtest profiles are believed to reveal his intervening cognitive *I* capacities, presumably heavily determined by heredity, *G*. The interview is also given, and is oriented to his personal history. Effort is made to discover the stress situations *S* that have harassed him. The interviewer believes that he also learns something about *D*, *G*, and *I* aspects.

These procedures provide a selected sampling of the infinitely complex body of behaviors, situations, genetic factors, and psychological states that constitute a person and his field. Implicit is the belief that knowledge about them can be secured from the verbal productions of a person; in most persons, however, even when they are calm and organized, the task of perceiving and conceptualizing such a matrix of events, forces, and conditions is quite formidable. These phenomena must, in addition, be represented symbolically by words, an art difficult even for the most highly trained. This approach poses a critical question: How salient and valid are verbal productions in representing the social phenomenal world of people, especially of the disordered?

Complex social behavior, distortions in which bring patients to treatment, usually are only incidentally verbal in character. Research studies of the salient interactions that comprise social living, I am sure will show them to be preponderantly nonverbal. If one looks at the kinds of social behavior cited as important in the great living religions or in other social sanction systems one will find that the social actions there described consist largely of nonverbal behavior, for example, in the Judaic Ten Commandments, the Christian Parables, and Sermon on the Mount, the Buddhist Eightfold Path and Ten Perfection, the Confucian conduct. How much, therefore, should we rely on data exclusively verbal? Diagnosticians and therapists attempt by cross-referencing "surface" verbalization to get at "underlying" dynamic systems that may control nonverbal behavior. The extent of such control remains to be established. In the office the objective is to make the perceptions, cognitions, and emotions of patients become conscious, organized, and accurately verbalized. This objective is desirable in its own right as a form of "supportive treatment." But it is not the same objective as diagnosing and treating the disordered social behavior that brings the patient to treatment.

Behavior disorders usually emerge at home, work, or play – in social situations. Here, therefore, is the place to observe their expressions. One promising move out of the office is the use as a diagnostic setting

of miniature real-life social situations. The constructions incorporate stress features that aim to elicit the patient's disordered behaviors or the intervening *I* variables that are presumed to control them. One thinks here of such setups as the Office of Strategic Services (OSS) "situations." the psychodrama, and group therapy sessions.

A modest immediate move would be to broaden the scope of stimulus materials currently presented to the patient in the office itself. I think I see a tendency to institutionalize present orthodox procedures. To block this dangerous trend toward accepting the responses to a standard test as sanctioned social norms in their own right, I seriously recommend a moratorium on rigid diagnostic practice and a policy of actively encouraging diagnosticians to devise and test any new methods that engage their theoretical and experimental fancy.

Look forward to the role of the diagnostician of the future. He should have available to him a great depository of office and lifelike test situations. The patient will first be put through broad, general screening procedures. His special pattern of performance on these will determine which of a large variety of funneling test situations are appropriate to his case. Guided by his pattern of performance on these, the diagnostician will then take actual field ratings of the patient's behavior in his real-life situations. I am not necessarily suggesting the telescreen of 1984, but if the patient is willing, the idea has merit, as would also be psychological FBI-type "agents" quizzing relatives, friends, neighborhoods, and employers.

The required multidimensional analysis of such a large number of variables, followed by a typological pattern analysis, is no longer awesome.[5,6] The electronic computer can handle this problem with dispatch. Is such a screening-funneling procedure fantastic? Certainly not, for such a complex routine is precisely the accepted methodology today in thoroughgoing medical diagnosis. We cannot expect the diagnosis of behavior disorders to be less complex than that of organic disease entities, for these physical disorders are often included as an integral part of the behavior disorders themselves. By these multidimensional procedures we may in due course be able to test or objectively to rate the variables now described in literary form in the *American Medical Association Bulletin* on psychiatric syndromes. To do so would require devising a variety of patient-relevant lifelike situations and rating scales, standardized for comparative purposes. The responses and ratings of a given patient would be symbolically arrayed as a metric profile, to be used by the therapist as a guide to treatment.

Let us now consider special research problems on the reliability and validity of diagnostic testing. Objective observations of each tested behavior should reliably differentiate persons. To devise a test of high reliability requires the taking of a large enough sampling of observations on the behavior to guarantee that each patient's standing on the total

score is stable. There is an old maxim that says that a test used for individual diagnosis should have a reliability coefficient of 0.95; for group comparisons it should be at least 0.50. Under the current stress of service loads and the desire for an inexpensive diagnosis, our present procedures would qualify only for a study of group differences, not individual assessment. Scores on elements of a Rorschach pattern or TAT protocol would not qualify even for group study; those of the MMPI probably would; and those of the Wechsler subtests certainly would, although only six out of the eleven have reliabilities as high as 0.8. The reliability of the interview is unknown, but probably very low. Our going procedures do not meet the standard for reliable diagnosis.

The validity of a given diagnostic test is its correlation with one or more of the "criterion" variables in our etiological equations. For example, tests of the behavior disorders *D* are valid to the degree that they measure the behavior distortions that send patients to treatment. It is more difficult to set criteria for generalized inner *I* dispositions. Thus far purported measures of such psychological constructs have not been shown to predict disordered behavior or, indeed, even to be general. In a recent as yet unpublished summary of a wide variety of researches designed to test the generality hypothesis, Donald Campbell and Donald Fiske examined many "multiconstruct-multimethod matrix" findings. They found little evidence of salient cross-situational validity of psychological constructs. These data continue to confirm, instead, the "specifist doctrine" that psychological dispositions, traits, and abilities are countless, are tied to specific situations, and show negligible transfer. Little research on the measurement of stress situations *S* has yet been attempted. The validity of such measures would be less difficult to establish because of the more objective character of stimulus situations. A forward step in diagnostic procedure will be taken when systematic efforts are made to define and represent objectively the types of stress situations that generate behavior disorders. The criterion of the gene-complexes *G* would be a representation of these hereditary materials. Since these complexes remain unknown in persons, no valid measures of them appear possible.

Psychological tests will be used increasingly to measure response effects of organic conditions that determine some behavior disorders. A close look at the relation between test and criterion shows that some tests are quite as respectable "predictors" as some standard procedures of the physician. Consider, for example, the testing of brain damage. The validation procedure involves, first, establishing a cut-off point in the array of test scores, above which a score is classed as "disease-positive." The second step is to select a criterion consisting of neurological patients divided by neurologists into two criterion groups, "organic" (brain-damaged) and "nonorganic" (functional). Usual correlation procedures are not likely to reveal the maximal predictability of the psycho-

logical test. The reason is that among the organic patients many may produce "false negative" scores (nonorganic), whereas among the nonorganic few show "false positive" scores (organic). Evidently, among the organically ill homeostatic mechanisms periodically "pull" the patient toward normal functioning, sufficiently so that if the test is administered at this stage he may earn a false negative score. The net effect is that a positive test score is valid, but a negative score invalid.[2] Usual correlation procedures do not apply. Large homeostatic variance among the behavior disorders is probably common. Its implications for diagnosis and treatment need to be studied.

Validation of a test devised to measure a component of a disorder requires a double-validity design. Consider the hypothesis that latent homosexuality is a component of schizophrenia. A test of latent homosexuality must be first constructed and its validity proved. The second step is to show that it is correlated with schizophrenia. We may short-cut the design, accept the test as a face-valid measure of latent homosexuality, and proceed at once to compute its validity coefficient with schizophrenic reactions. If some validity is discovered, we would not thereby prove, however, that the test also measures latent homosexuality. The double-validity design indicates why we must realistically face the fact that psychological tests will have low validities as predictors of disorders. The reason is, first, that among patients suffering from a given disorder the postulated component will be only a partial determinant in all or most of them, or highly salient only in a few. Second, a component such as latent homosexuality is itself almost certainly determined by a host of its own subcomponents of which the test samples but a few. The validity coefficient is a part-whole (and a very large whole) coefficient, thus necessarily low although the original hypothesis is correct.

The prime objective is to devise tests that will discriminate between the disorders or their components. To demonstrate discriminant validity requires that the base population on which the test is validated be the patient population. A valid test of schizophrenia should differentiate schizophrenics from other patients, one of the brain-damaged from other neurologicals. If, following a frequently used design, one merely proves that a test differentiates schizophrenics from normals, the result is ambiguous, for no precise claim can be made that it uniquely measures schizophrenia – it might do as well with other disorders. Prediction over "chance" by a discriminantly valid test would be shown by a large excess in yield of disease-positive scores among schizophrenics over the base rate of schizophrenics in the patient population.

Treatment

Let us restrict the problems of treatment to those to which the psychologist can most appropriately contribute. We leave aside, therefore,

matters of treatment of organic conditions and the use of shock, drugs, and other such medical methods.

A patient comes for treatment when he or others believe that he is or may become grossly deviant in the social behaviors expected of him. The object of treatment (ignoring important points of social ethics) is to remove the deviance. To do so would require altering the causes, *S*, *G*, and *I*. Nothing can be done to alter his genetic constitution, *G*, even if it were known. Treatment is confined to *S* and *I*.

Current practice consists largely of a cooperative verbal onslaught by therapist and patient on the inner *I* dynamics believed to be the psychological root of the patient's troubles. The object is by verbal manipulation to alter the manifest and latent dispositions so that the patient experiences supportive treatment and also, it is hoped, becomes better able to deal with the stress situations of his social life outside the therapy situation. Implied in such treatment is the belief in generalized inner dynamics that, when changed in the therapy setting, transfer to outside social situations, and in a superior competence of the patient to conceptualize, verbally symbolize, and manipulate these complex dispositions across situations.

Psychological experiments on learning, concept formation, and emotional conditioning have established the fact that "inner" dispositions do exist, operationally speaking, but in very large numbers and wedded to the types of situations in which they have been learned or elicited by maturation. If they are to be modified by treatment, the place to do so would be the situations that elicit them. For behavior disorders the appropriate sites are the stress situations that generate the disorder. Future research will show, I suspect, that, although office therapy may provide some comfort and reorientation of patients, the greatest improvements will occur when the psychologist goes with the patient out into his real-life situations and assists him there to relearn or learn anew how to cope with his stresses. New basic research will be designed to discover the applicability there of "laws" of perception and learning developed in the psychological laboratory and to test in the laboratory conceptual models created from field study. On the patient's home ground, the psychologist may serve also as an instrument to facilitate altering stressful elements or realistically to suggest the substitution of other situations with less tension-producing features. Some moves toward treatment *in situ* are seen in the rise of group therapy, psychodrama, the out-patient clinic, the "open" hospital, and the psychiatric community.

The emphasis here is on social psychological analysis and management of behavior disorders, an untouched research field. Persons live their lives in different group substructures of society.[5] We need to know how stress situations, heredities, and psychological dispositions differ among these substructures. If we conceptualize these subgroups of people as different selected samplings of stress situations, genotypes, and

intervening psychological variables, then within this framework we may develop a discriminating knowledge of the causes of their disorders and and of the most effective means, verbal and nonverbal, of diagnosing and treating them.

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THE NEW YORK ACADEMY OF SCIENCES

SECTION OF ANTHROPOLOGY

SOCIAL SCIENCE AND FOREIGN POLICY*

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The physical life of the average individual in the so-called developed countries is to a large extent controlled from beginning to end by science. Such an individual may even be conceived by artificial insemination; in any case, the chances are at least three to one that his entrance into the world will be effected through the application of obstetrical science. Beginning in infancy and throughout the rest of his life, he is vaccinated, inoculated, diet-controlled, and exercised by various applications of science. His teeth may be straightened, his eyesight corrected, his ailments treated, and his demise finally supervised by scientifically trained practitioners of a wide variety. Yet the average man also lives as a member of a society and orients and maintains his life by following a set of patterns provided by the culture of that society, participating in social institutions and aiming at certain valued goals or aspirations. Such a set of life ways and associations is called a sociocultural system. In the contemporary world the discrete systems are politically organized as nations which, in turn, may be grouped into blocs or alliances. Despite the plethora of scientific attention to the physical needs of the individual, the possible applications of science to the national and international aspects of his life are frequently ignored.

Differences between nations exist with respect to the details of their customs and institutions, but when disputes and conflicts arise they usually have to do with real or apparent incompatibility between national goals and the instruments for their achievement.

For nearly one hundred years following the Congress of Vienna in 1815 diplomacy was the principal method for settling disputes and avoiding conflict. Competition between rival nations also permitted the pursuit of diverse aims without open conflict. However, both traditional diplomacy and international competition involve mutual agreement upon rules that restrict the activities of the opponents. When such agreement could no longer be reached, resort was had to war.

The recent development and application of certain types of science, however, have created a situation in which all-out use of the most advanced weapons of war is rationally unthinkable as an instrument of national or international policy. When, as now, both potential opponents

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are equipped with thermonuclear weapons to retaliate on a scale of physical destruction never before realized, a stalemate has been achieved at least temporarily. Enormous sums of money are provided and formidable concentrations of scientific and engineering personnel are employed by both sides in an attempt to maintain this dangerous equilibrium.

In the Eighteenth Century Thomas Hobbes, in his book *Leviathan*, expressed a view that at the time was regarded as rather quaint. "War," he wrote, "consisteth not in Battle only, or in the act of fighting; but in a tract of time, wherein the Will to contend by battle is sufficiently known.... The nature of war consisteth not in actual fighting, but in the known disposition thereto during all the time there is no assurance to the contrary." In the early Nineteenth Century Karl von Clausewitz, the well-known Prussian military theoretician, stated in his book *On War:* "War is not merely a political act, but also a political instrument, a continuation of political relations, a carrying out of the same by other means." To paraphrase von Clausewitz, peace is a continuation of war by other means. For such a concept one might coin the seemingly paradoxical term peaceful warfare.

It is evident that the second half of the Twentieth Century confronts any nation or alliance of nations with a situation novel in modern history. The employment of even the instruments of conventional warfare must be strictly and voluntarily controlled lest their use develop into a thermonuclear holocaust. Once this is conceded, it would seem to be no more than common sense to conclude that, whether a nation seeks to wage "peaceful warfare" or aims at the establishment of a true peace, appropriate scientific resources of the most advanced types available must be marshaled and applied. Furthermore, just as the threat of thermonuclear war demands that each side support basic research in order continually to advance the boundaries of knowledge in physical science, so also this new type of situation logically requires the constant deepening and expansion of the types of science applicable to it.

The sciences to which I refer are, of course, the social and behavioral disciplines. Thus far, in the United States at least, these disciplines have received remarkably little attention in connection with the formulation and execution of foreign policy. The sums appropriated for their researches by either governmental or private sources are miniscule as compared to those made available for the physical and life sciences, and inducements offered young men and women to prepare themselves in these studies are insignificant in comparison with those offered in other scientific fields. However, it may well be that the survival of ourselves and our friends in a peaceful war will depend upon the proper development and application of this sort of knowledge. Certainly, if a true peace is achieved, the improvement of our sociocultural system as a way of life and social organization for the fulfillment of the highest human potentialities will be greatly enhanced by such learning. It is for such reasons

that I take this occasion to touch briefly upon certain aspects of social and behavioral science in the present context.

First, something of a personal note. Since I am myself a social scientist – one of the type known as anthropologist – my present arguments might be viewed as special pleading: as an effort to take advantage of the present situation to enable my colleagues and myself to advance our material interests. I doubt that our friends in other fields of knowledge subscribe to such sordid views, but I have heard whispers to this effect among laymen. In reply I should say that most anthropologists probably look back with nostalgia to the days when they felt no responsibility for national or international problems, when their knowledge of cultural structures and processes could be neatly and exclusively applied to relatively simple primitive tribes and peasant communities. We were not luxuriously financed, but few of us had great difficulty in obtaining funds to support ourselves and our students in the relatively small-scale projects we were accustomed to undertake. The fact is that to many an anthropologist, sociologist, and psychologist the prospect of becoming involved in world problems and complicated organizations is uncongenial, if not somewhat frightening. The expanded application of these disciplines to national and international policy may well require some re-education of the practitioners themselves. Accordingly, it is not for the aggrandizement of the social and behavioral sciences and their personnel that I plead, but rather for assistance to enable them to attain broader objectives.

One of our principal deficiencies in the employment of the social sciences in foreign affairs has been their lack of coordination. I do not argue that we should establish some sort of authoritarian organization of social science, but we sorely need the holistic view. In making policy concerning a certain nation or region our experts should be provided with information and analysis that cover the whole situation, with the various parts properly placed in functional and structural context. One of the Communist instructions puts the matter this way: "The Party must be prepared to use any change in a situation which is favorable to the ultimate goal. Since we do not know and cannot know which spark...of the innumerable sparks that are flying around in all countries...will kindle conflagration...we must...set to work to 'stir up' all, even the oldest, mustiest and seemingly hopeless spheres...." Recently in Ecuador I had occasion to see something of the programs of various United States missions in health, education, and agriculture. Each reported to its own headquarters in Washington, D. C., and a certain lack of coordination in the field was evident. One of my informants, a highly placed Ecuadorean government official, remarked that such an apparent lack of common planning and direction left many *grietas abiertas* (open cracks), into various of which, as he pointed out, the Communists did not hesitate to penetrate.

Cultural anthropology cannot substitute itself as a source of supply for data and analysis of other specialized disciplines, but it has developed a theory and a technique for viewing and analyzing human sociocultural situations as wholes. There is no reason to think that this point of view and skill could not be meshed with those of other specialties in the basic planning and execution of policy.

Foreign policy, to my way of thinking, is or should be largely concerned with the "correct" perception and interpretation of the over-all human situation in any nation or region with which it is concerned. Our policy experts have frequently exhibited errors in this respect that in the future might be reduced by a more systematic use of social science. One of the principal features of correct perception is the ability to see things as the other nation or region does, or at least as its leaders do, in order to be able to interpret their motivations and possible decisions. The world view of foreigners is controlled chiefly by the value systems of their particular cultures, just as we are controlled by our own cultural premises. A frequent block to correct perception and interpretation of foreign systems is culture-binding: our policy makers project upon the foreign nation or region their own cultural images and expect the foreigners to see things likewise. There exist techniques that, while not 100 per cent perfect, are adequate for discovering and interpreting the controlling value complexes of foreign peoples and comparing them with our own preoccupations. In this connection, we are told that since the end of the last war our government has developed a very efficient system of gathering "intelligence" abroad, but intelligence itself must be evaluated, and the types of information to be collected should be covered by central planning so that nothing significant is overlooked. In both of these functions up-to-date social and behavioral science, properly coordinated, could increase efficiency.

Other inadequacies of our foreign policy are occasionally attributable to "time lag." This is based to some extent upon the assumption that the situation as described in the past holds true today. Our policies often seem to be deficient in predictive power, and this may be due to to overreliance on history, political science and economics combined with insufficient attention to possible findings of sociology, social psychology, and cultural anthropology. The former three disciplines, while indispensable, depend primarily upon written or printed documents for their data, and they tend to deal with formal, established, traditional institutions and practices. However, one of the outstanding features of the present world situation is the world-wide social revolution that is taking place, at least in the underdeveloped countries, and the most embracing aspect of which is a series of break-aways from traditional patterns. There is need to embody in foreign policy work the newer social sciences that are theoretically and technically equipped to discover and interpret trends going on behind the facade of public institutions and

pronouncements. For example, changes in social structure and in the aspirations of newly emerging groups often result in important political changes that our policy must be prepared to anticipate rather than merely adapt itself to after they have become *faits accomplis*. The State Department did not publicly perceive until ten troubled years later that the Guatemalan social revolution, beginning in 1944, was in large part the product of a newly expanded, liberal, frustrated middle segment of society that, at the time, was not Communist-indoctrinated but demanded changes in the government in accordance with its aspirations for a better world. Likewise it would seem that the department did not perceive that in Egypt a new generation of military officers, mainly the offspring of *petites fonctionnaires*, had been educated in the army schools and had constituted themselves a power group that would take over the government. These young colonels had also developed aspirations and views that did not coincide with those of the older power holders: the pashas and the effendi.

It is sometimes said that Soviet policy is also "culture-bound," because the Russians claim that Marxism provides a "correct" scientific perception and interpretation of capitalism and because of their faith in the inevitable triumph of communism throughout the world. However, the Soviets do not seem to allow Marxian "science" to interfere with basic physical science and the development of weapons, nor are they evidently content passively to await the victory of Marxism. Marx perhaps, as is sometimes said of God, "helps him who helps himself."

Without going into further details, I believe that most observers would agree that the United States' foreign policy has been formulated and, on the whole, carried out by sincere, earnest, and dedicated men. Such errors as have been made are not the products of character or personality defects. However, the United States is in a situation where fewer and fewer errors can be permitted. It is the present suggestion that a more systematic use of available social scientific resources, plus an expanded program for the support of their development, would help to reduce errors, and may in fact be absolutely necessary for the security of ourselves and our allies.

Let us turn briefly to a few possible aspects of peaceful warfare, or that type of international conflict whose objective is conquest without the use of military weapons as its principal instruments.

"Conquest" may be considered the political control of one people by another or of one sociocultural system by another. However, in the real world there are various degrees of conquest and various steps along the way. For example, complete collapse of a nation's system of social organization and culture may precede the victor's establishment of control, as was the case with several culturally stripped simple Indian tribes of North America, such as the White Knife Shoshone of Nevada. Or the victor may be content merely with collapsing the political portion of the

system and substituting personnel of its own choice therein, leaving the rest of the cultural and social organization in functioning, if somewhat modified, form. Despite the slogans of "unconditional surrender" and "total victory," this seems to have been the effect achieved by allied victory over Germany, Italy, and Japan in 1945. Furthermore, at a given point in an international struggle, the immediate objective may be merely to weaken the opponent as a power or a threat. This may be done not by direct attack, but by causing defection among its allies or disorganization in its "sphere of influence" or area of support.

If one examines the record, there seems to be no case within the past one hundred years of a modern sociocultural system that has been totally collapsed even by the use of force. In 1871, following its six-year war with the triple alliance (Argentina, Brazil, and Bolivia), Paraguay suffered a collapse of its former social organization, due in part to the fact that reportedly about 80 per cent of the male population between ages 20 and 60 had been killed. However, the victors did not take permanent control of the country, and it was not stripped of its culture. Poland has suffered several severe alterations in social and political structure, the latest as a result of World War II, but one of the notable facts of modern history seems to be that no one, even Hitler, has succeeded in stamping out the core of Polish culture. In other cases, as for example the two German occupations of Belgium, the victor attempted a species of indirect rule, retaining the native structure and replacing the control personnel with other natives more or less amenable to the occupying power. This is roughly analogous, although accompanied by more use of force, to the indirect rule established by Great Britain over conquered native tribes in Africa. In Norway during the last war this procedure was called quislingism.

Thus it would seem that neither the complete extermination of a people nor a total collapse of a modern complex sociocultural system has been accomplished in recent times even as the result of physical warfare. Nor does it seem that such results are realistic aims of war in the modern world—whether the struggle be "hot" or "cold." Although there may be hallucinators in Moscow who fancy a world purged of North Americans, the physical extermination of our population would leave a huge pesthole of more than three million square miles whose menace to the health of the victors alone would probably exceed its danger as an organized enemy nation. The same considerations apply to any similar dreams on this side of the Iron Curtain. Likewise, the complete or near-complete stripping of the population of any modern nation of its culture does not appear to be a practicable objective.

If there is to be a peaceful war at the present time between the Soviet bloc and the Free World, it would seem that its principal battlegrounds will be the underdeveloped regions and weaker nations. We already see evidence of such a struggle going on in the Near East, in

southeast Asia, in Indonesia, and in Latin America. Africa south of the Sahara probably also will in due course become a battleground.*

Let us consider, as an example, certain general aspects of the policy of winning in one of these areas, namely, Latin America. Since 1823 it has been a basic tenet of United States policy that no outside power be allowed to establish itself in Latin America, but little attention has been given to the possibility that one or more nations of the area might transform themselves into allies or sympathizers of an outside power uncongenial to the United States. Armed intervention by the latter has been thought to be inevitable. However, the more the United States intervenes with arms, even though indirectly, as in the Guatemala Liberación of 1954, the more unfriendly the Latin Americans become. If it is desired that the area remain solid with the Free World, what can be done, and how can social science be of service?

Much melodramatic writing has appeared about the use of espionage, undercover agents, propaganda, bribery, sexual seduction, and other manipulations of persons, deceit, and obfuscation on national and personal levels, and the use of backstage economic weapons. Perhaps such back-alley tactics are necessary in certain eventualities, but sound over-all policy will not allow such situations to occur with any frequency. Winning strategy will be based upon reliable and comprehensive understanding of the sociocultural situation and upon adjustment of our own policies so as to swing trends and developments into line with our own goals and those of free mankind. Conversely, one can help oneself to lose in this kind of contest, as in the Japanese wrestling game called judo, in which the defendant turns to his own advantage his opponent's false moves. If we insist upon rigid maintenance of the status quo and refuse to recognize the legitimacy of new aspirations, the opposition will take advantage of such mistakes for its own ends.

Several factors need to be taken into account in Latin America:

First, a reorganization of society has been going on. The most important result has been the emergence of new middle status segments and the appearance of a new upper status group of self-made men or their descendants. The proportional numbers of these elements vary from one country to another, but are significant everywhere.

Second, these new elements have borrowed from the outside and developed on their own account certain new values that they are endeavoring to integrate with the traditional values of their culture.

Third, political and military power has tended to shift from the conservatives and the old aristocracy to the new segments of society, and they have endeavored in various ways politically to implement the new goal values. The Mexican revolution was accomplished before that of

*On April 21, 1958, the so-called first conference of independent African states adopted a policy of "nonentanglement" with either of the rival great-power blocks. This indicates in itself that "Free Africa" has already become a "battleground" in the peaceful war.

Soviet Russia and is widely admired throughout the area as an authentic expression of Latin American social and political reform. Among the patterns for which political instrumentation is sought or has been achieved in most of the countries is division of the large landed estates, national control of basic natural resources, government responsibility for old-age pensions, health and accident insurance, government support for free enterprise and the small entrepreneur through credit facilities and planned economic aid, universal education, provision of technical and clerical education, in addition to the traditional humanistic and professional courses, and free suffrage for both men and women.

A fourth factor is the rise of nationalism plus a vague solidarity in "anticolonialism." Although many thoughtful individuals are not influenced, there is a strong "antiyanki" sentiment and a stereotyped belief that the United States represents "imperialism."

Fifth, the leading elements stand for the preservation of certain of the traditional values of their culture and work for their integration with the newer patterns, in the hope of bringing their people into the modern world without making them carbon copies of anything else. Among these are what are often referred to as values of the person, the inner uniqueness and dignity of each human individual. Also emphasized are values of the spirit—transcendentalism—*algo más allá*, as they say in Spanish. Another is the good of emotional expression. Even the ordinary man has the right to be a poet, and life would not be worth living without the *chiste*, the joke, the neat turn of phrase. Nor has one savored it fully if one does not occasionally give way to a certain sweet sadness—*tristeza*.

Sixth, workingmen are being organized everywhere, usually under the leadership of educated members of the growing middle mass. With the diffusion of education and the spread of the mass media of communication, we may expect to see the lower classes become increasingly influential in the national societies.

Finally, in many countries a continuing struggle has been going on between "dictators" and "the people." I do not have the time to analyze this in any detail here. Investigation shows, however, that in the past twenty-five years, at least, no Latin American dictatorships have rested for any length of time upon the support and consent of the majority of the population. Thus the accusation is frequently made against the United States that it supports dictators in opposition to the will of the people.

A close examination of these factors in the Latin American situation would show that none were originally Communist creations. Nevertheless, no extended discussion is necessary to indicate how the Communists in various ways take advantage of them in their own interests and against those of the free world. some of the new values of the Latin Americans appear "radical" to conservative members of the United States government, and some of the older values, such as emotionalism, for example, look silly. As long as our policies appear to reflect only indifference or opposition,

the Communists have little difficulty in convincing many Latin Americans that their aspirations are really identical with those of communism.

Taken together, the making and execution of foreign policy are, to some extent, an art, but such an art, to be successful, must also be reliably informed and directed. I believe that the systematic and comprehensive use of behavioral and social science can form the basis of policies that will not only argue propagandistically, but will also convincingly demonstrate by action, to the peoples of the underdeveloped areas emerging into the modern world, that alliance with the free-world systems offers them better prospects for the realization of their aspirations than submission to Communist imperialism. However, we must never forget that the best defense in a peaceful war of this kind is an unremitting attempt on our part to improve our own systems as ways of life for the realization of the highest human potentialities anywhere. In this endeavor, inspiration, intuition, and art, although valuable, are not enough. The human sciences should be called upon to contribute their part.

From a scientific point of view peaceful warfare may be placed within the general category called directed cultural change. Anthropologists and other social scientists have made extensive studies and developed certain basic principles in the field of directed cultural change. Anthropological studies have hitherto been focused primarily on directed or planned changes attempted in the cultures of dependent or colonial peoples, or of so-called underdeveloped areas, or of what are sometimes called backward segments of our own population. Such situations are perhaps somewhat more simple than those involving "peaceful" aggression between major powers or groups of powers, but there is no present reason to believe that the processes of change and the techniques employed differ fundamentally in principle. Furthermore, attempts to change certain parts of the culture of the United States and other complex Western sociocultural systems are continuously going on, although often under other labels. The advertising industry, various political groupings, and a host of nonpolitical and noncommercial reformist organizations are continuously at work trying to alter one or another aspect of our cultural behaviors or values by peaceful means. Their techniques and results have been studied by social psychologists, motivation psychologists, sociologists, public opinion pollers, political behaviorists, economic behaviorists, historians of contemporary art and literature, and other such investigators. Such studies are usually undertaken from a variety of points of view and seldom are coordinated to produce an accurate image of what is happening to the entire sociocultural configuration. However, they have produced a great volume of observations and some validated theory concerning what are actually directed or controlled attempts at culture change in modern complex systems. I have not mentioned the large amount of data collected and analyzed on the civilian reactions to disasters brought on by physical agents in both war and

peace because, presumably, physical disasters are precluded from the weapons of peaceful warfare. These studies, however, throw much light upon the basic structural and functional features of modern communities, if not of total national systems. Thus, if we are in a period of cultural change directed with hostile intent from foreign centers, we are not entirely devoid of scientific and humanistic resources for coping with it. These resources, however, must be recognized publicly and should receive national support adequate for their continued development and coordination.

There is, to be sure, some talk about social or behavioral science and its applications to the present crisis, but this tends to be of a particularistic or single-angle type. One hears, for example, that everything will be solved if only the economic system of a certain nation or area is brought into proper function. Then, again, we are told that the whole trouble has to do with the political structure. Or, perhaps, the deficiencies can be corrected if only the technologies are brought up to date, whether these technologies concern health, education, agriculture, extractive or fabricating industries, administrative techniques, or other such areas. The advocate of such a point of view usually insists that his particular technology is the answer to everything.

Anthropology long ago developed a method of observing and analyzing sociocultural systems as wholes. It was developed, however, with respect to relatively simple tribal and peasant systems. In considering modern complex national and regional systems, the cross-cultural point of view requires, not only anthropology, but all the other specialties dealing with human social and institutionalized behavior. There is every reason for the mobilization of both government and private resources to support such integration in the interests of our survival.

THE NEW YORK ACADEMY OF SCIENCES

SECTION OF OCEANOGRAPHY AND METEOROLOGY

CLOUD EXPLORATIONS OVER AFRICA*

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During late October and early November, 1957 I flew, rode, and walked over about 13,000 miles of southern Europe and Africa, crossing the equator four times, viewing strips of both the west and east portions of Africa from airplane windows, and covering small portions of the land by auto and afoot. I was guided during this exploratory visit by Capt. Malin Sorsbie, C.B.E., of Nairobi, Kenya, who is now president of our foundation. To a person with an avid interest and curiosity in atmospheric physics and chemistry, such an exploratory trip is highly stimulating. Only in such a manner can one begin to comprehend the scope of the atmosphere and the complex interactions that combine to produce weather.

Because of its vast deserts, rain forests, mountains, wind gaps, freshwater lakes, inland rifts, adjacent seas and oceans, and because it represents the largest land mass of the world athwart the equatorial zone, Africa presents a challenge to all explorers. Many fascinating problems confront the scientist interested in Africa's atmosphere and its contents, and in the reactions that take place between it and the sun, land, and water of that continent and its peripheral areas.

The Intertropical Convergence Zone

Flying southeast on a line from Kano in Nigeria to Salisbury in Southern Rhodesia, our plane penetrated the spectacular cumulus and stratus cloud system comprising the so-called intertropical convergence zone (ITCZ) of the fliers who travel that area. This proved to be extremely interesting to me, for only two months previously I had been studying the glaciation of large and small cumulus clouds in the northwestern United States,[1] and it was apparent that, although the concentration of natural ice nuclei in that region was obviously rather low, enough had been present to initiate some ice crystal formation. Although some subcooled cloud existed here also, as evidenced by the light icing that formed on the tail fins of the plane, sufficient ice nuclei were present to produce snow showers from both the side turrets of the large cumulus and from the thicker layers of stratus clouds extending outward from the upper portions of the convective cloud masses.

When passing through its counterpart over eastern Africa in northern Uganda and the southern Sudan three weeks later, much more snow was

*This paper, illustrated with colored motion pictures and lantern slides, was presented at a meeting of the Section on March 25, 1958.

in evidence – although even there a shortage of nuclei was evidenced by the graupel that could be seen aggregating in the ice deposits on the tail fins, by the whiteness and size of the particles in the air, and by the absence of a reflected undersun in the snow areas through which we passed. It was an interesting experience to fly through heavy snow showers directly over the equator. The ITCZ was a collection of cumulus nimbus of fearsome appearance. However, by radar navigation the pilot threaded a course through the huge towers and storm areas to avoid any encounter with turbulent air. As in its counterpart along the western side of Africa, the air north of the convergence zone rapidly cleared of cloud and, just before darkness prevented further observations, all clouds had been left behind.

Large Subcooled Cumulus Over the Southern Congo and Rhodesia

Leaving the ITCZ during my southeasterly flight down the west side of Africa, we crossed the Congo River at the equator and headed directly for Salisbury. South of the Congo we observed small cumulus clouds quite similar in appearance to those that form in the area where the trade winds blow. These occurred in long, curved cloud streets of interesting pattern. They gradually dwindled in size as the country became more barren until, finally, the region encountered was cloudless. For the next several hundred miles the country was quite flat and featureless; the air was filled with a smoky haze. Over the southern Belgian Congo scattered bush fires were seen; these became increasingly common as we approached the Katanga area and Northern Rhodesia. Between Katanga and the Zambesi River, we passed among huge towering cumuli. These showed evidence of strong convection, low bases, high liquid water content, and a complete absence of ice nuclei. For more than 400 miles we encountered these massive clouds growing singly and in clusters. Although many of them towered far above the plane, which was flying at about 22,000 feet, not a trace of glaciation, virga, snow, or rain could be seen in any of them throughout the area observed. Many of these clouds were producing at their summits great streamers of false cirrus as they presumably reached the homogeneous nucleation temperature at about -40° C. As normally happens under such conditions, the crystals that formed were so small that they showed little or no tendency to fall to lower levels. Careful attention was directed toward looking for evidence of ice nucleation or water coalescence effects in the lower portions of the clouds. Not one example was seen.

It is quite possible that this represented a classic example of overseeding by condensation nuclei.[2] Under such conditions, when the atmosphere contains unusually large numbers of good condensation nuclei (as were present due to the heavy smoke concentrations up to 20,000 feet), the cloud water is so effectively distributed among the

large numbers of nuclei that there is little chance for the coalescence process to start. Certainly these clouds were entirely different in behavior from those seen just north of the equator, although they seemed larger in all dimensions.

Silver iodide or dry-ice seeding would certainly produce striking changes in such clouds and would probably lead to important changes in the moisture pattern of the area underneath, provided such cloud behavior patterns are of sufficient occurrence to be so treated regularly.

Thunderstorms in South Africa

On October 25, 1957, while at the University of Witwatersrand in Johannesburg, I had an excellent opportunity to observe the development of typical thunderstorms of the type that have been studied so effectively by Malan.[3] Here again I was impressed with the apparent lack of natural ice nuclei and by the ineffectiveness of other precipitation-forming mechanisms in the clouds observable. Although clouds developed to towering stages by midmorning and assumed spectacular proportions by midafternoon, little if any phase change occurred even in the higher reaches of cold clouds having temperatures certainly as low as -30° C. Toward evening the sky contained much false cirrus at very high level and, by evening, violent local thunderstorms were occurring in all directions, especially in the region of the Drakensberg, south of Johannesburg.

Observations of natural nuclei concentrations made by Mossop[4] indicate the relatively low level of ice nuclei observed in several parts of South Africa. The entire region possesses excellent opportunities for experimental meteorology and atmospheric physics.

While traveling by air from Durban in Natal, South Africa, to Dar es Salaam in Tanganyika, East Africa, I was able to observe an interesting display of jet-stream clouds.[5] These included most of the forms I have associated with this atmospheric phenomenon, including cirrus, cirrocumulus, alto-cumulus billows, and waves. The clouds observed on October 26, 1957 were most striking in the vicinity of Durban, suggesting a flow from WSW. Wave clouds to the lee of the Drakensberg and over the Indian Ocean were very similar to those commonly seen in the middle latitudes of the northern hemisphere when the jet is wide and of relatively low velocity. After traveling more than 2000 miles we landed at Dar es Salaam, where I had an unequaled opportunity of observing the sky over a period of ten days from a vantage point on the shore of the Indian Ocean.

Cloud Forms of an Easterly Wave

Just before sunset on October 28, a spectacular series of cirrus clouds moved in from the west. The next morning the cirrus was no longer visible, while a reversal of flow was indicated by movement of alto-cumulus

at middle levels from ESE. Light rain fell from warm cumulus in the early afternoon, followed by a clearing trend that produced a magnificent yellow sunset. The atmosphere was brilliantly clear that evening, indicating the arrival of a new air mass. At sunrise, large cumulus clouds were developing over the Indian Ocean and moving toward land from the ESE. These became very large during the morning hours and displayed glaciated anvil tops through breaks in the overcast. Heavy curtains of rain could be seen over the ocean as intense showers developed. At about noon a well-defined squall line, similar in appearance to sharply defined cold fronts in the middle latitudes, was seen moving toward shore from the sea. This swept inland with a velocity estimated at 20 to 30 knots. As it passed by, heavy rain showers of an intensity rarely experienced in middle latitudes followed. These heavy showers continued at frequent intervals for the rest of the afternoon and evening; the rain then gradually decreased and ended during the night. Thunder and cloud-to-ground lightning accompanied the more intense showers. Dawn disclosed scattered, towering cumulus, the most striking display occurring in a cloud street extending from the island of Zanzibar to the coast near Bogamoyo. In the areas between the large cumuli, large, scattered patches of rippled alto-cumulus could be seen. At about noon a flow of thin cirrus was again seen to develop at high level and to move in from the west. The cloud system related to the so-called "easterly wave" thus completed its cycle.

Two successive waves followed, and an amazing similarity of cloud forms and atmospheric behavior occurred at about the same intervals of time. At Dar es Salaam I obtained an excellent series of time-lapse movies showing the various cloud patterns that occurred during the development and passage of an easterly wave.

Mother-of-Pearl Clouds Near the Equator

It has always been my understanding that mother-of-pearl clouds were some of the more unusual high level forms of clouds characteristic only of the high latitudes.

On two separate occasions, within a degree or two of the equator, I observed and attempted to photograph spectacular examples of these interesting wave clouds. These were seen at sunset and for a short time thereafter. The clouds showed typical wave structures; a few of them were of nearly pure lenticular form. They seemed to possess a uniform color ranging from light blue to green and red. None of those seen were as large as the noctilucent forms illustrated by Ludlam,[6] but were more like some of the nacreous clouds photographed by Störmer, an example of which has been published by Grant.[7]

On November 9, 1957, a group of these clouds was seen from the Nairobi Game Park in a direction that would place them to the lee of Mt. Kiliman-

jaro if an easterly high level flow occurred. A glider pilot of the Nairobi Flying Club had previously told me he had seen such clouds in the same locality.

Another assemblage of mother-of-pearl clouds was seen on November 12, 1957, during and after sunset while I was flying at 20,000 feet from Entebbe to Khartoum shortly after passing through the Intertropical Convergence Zone at about 3° N. Throughout the night no further clouds occurred along our path, which followed the Nile northward to Khartoum and then headed for Bengasi in Libya on the shore of the Mediterranean.

Observations Over Southern Europe

Interesting warm cumulus covered the sea and portions of southern Italy when the sun rose the next day. The mountains of Italy were seen in sharp relief, with haze and local fog in some of the valleys. Over Rome the first evidences of a jet stream were visible in the form of a mass of cirrus streamers coming from the direction of the French Alps. These became more striking as we proceeded across the Po Valley, which was covered with a solid blanket of smooth-topped stratus. The mountains of southeast France were covered with a fresh blanket of snow and protuded above the stratus, looking like jagged icebergs floating in a frozen sea.

West of the divide, between the Po and Rhone drainages, another solid blanket of stratus existed whose top was about one thousand feet lower than the clouds on the eastern side. Masses of cloud streamed westward over the mountain ridges and through the passes to appear as rivers and waterfalls. Mont Blanc, with its glaciers and jagged rocks surrounded by its lesser peaks, afforded a fitting climax to a unique experience, the memory of which persisted through the anticlimax of a London submerged in a damp, cold, drizzling rain falling from a thick layer of stratus engulfing the British isles.

The Preparation of Cloud Stereophotos

Throughout all phases of my atmospheric explorations in various parts of the world I make it a practice to take stereophotos of clouds and interesting terrain. This is easily accomplished with an ordinary 35 mm. camera, using an extremely simple technique. If done from an aircraft, it is desirable, if at all possible, to photograph in directions away from the sun. For such purposes, a window seat in the rear of the plane on the side opposite the sun should be obtained if possible. In making a stereophoto pair, the first picture (subsequently to be viewed by the left eye, if the photograph is obtained from the port side) is carefully framed and, as the shutter is snapped, a feature close to the center of the field of view is noted. If traveling on a large aircraft at a speed of about 300

knots, the second picture of the stereo pair should be taken after waiting 10 to 15 seconds, photographing the scene so that the feature near the center of the field previously noted is again located in the same relative position of the viewing frame. When such scenes are later viewed in a stereoscope, an entirely new dimension is revealed. Since the impression obtained is as though one's eyes were from 500 to 1000 feet apart, the shape and depth of clouds are seen as entirely new features of the atmosphere. Cirrus cloud structures generally produce the greatest surprises.

In taking such pictures, care must be exercised that only one picture includes nearby objects such as the plane's wing or tail. The lens should be held as close to the window as possible without touching it. A shutter speed of 1/100 sec. is recommended.

The airplane presents the meteorologist with a magic carpet from which he can observe and study the "why" of the weather. A knowledge of atmospheric physics permits him to have stimulating experiences wherever he goes; it is the one compensation of air travel, which otherwise can be tedious and uninteresting. With the advent of jet transport, such experiences will become even more commonplace. The knowledgeable meteorologist has much to anticipate during the next few years.

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DIVISION OF MYCOLOGY

THE PATHOGENESIS OF HISTOPLASMOSIS*

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In the area of high endemicity of histoplasmosis[1] in which we live, 80 per cent or more of the population react positively to the histoplasmin skin test at the age of 18.

This indicates present or, more often, past infection with the organism *Histoplasma capsulatum*. The latter was found in the soil first by Emmons[2] and, subsequently, by many others.[3-5] The spore-containing dust of the soil is inhaled and produces a primary focus in the lungs and satellite lymph nodes (primary complex).[6] The primary infection is commonly asymptomatic and, most often, the primary complex heals and becomes calcified. In fact, the calcification is so massive – even if often stippled – that it becomes a trademark of the histoplasmic infection.[6,7] We have demonstrated that most primary foci of 5 mm. and above are histoplasmic in origin, and similar behavior is found in the lymph nodes pertaining to the primary complex.[6,7]

Apparently, therefore, we are dealing with a benign disease that, much more often than not, is self-limited. However, it is well to be aware that the primary infection very frequently disseminates by the bloodstream, even in the cases that appear to be asymptomatic or have so few clinical manifestations that they are mistaken for a common cold or similar minor attacks of "flu." The dissemination by blood is demonstrable even after years in the form of typical splenic calcifications,[8,9] which can be only hematogenous in origin.

In the presence of a calcified primary complex the calcified splenic and hepatic foci indicate that they are probably of similar age. We have, then, an apparently benign disease that, in an overwhelming majority of cases, is self-limited but that, at the same time, is invasive enough to produce metastatic necrotic foci in the spleen and, less frequently, in other organs such as the liver. One can assume that, at this time of early dissemination, occasional emboli are thrown to the adrenals, to the brain, or to the bones, and this then may give origin to the development of caseous "histoplasmosis of organ systems." In the endemic area Addisonian disease is probably produced much more often by histoplasmosis than by tuberculosis or any other granulomatous disorder.[10] Obviously the mobilization of antibodies will be the decisive factor in succumbing to, or curtailing, the disease process. In infants the danger of lethal or,

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TABLE 1

HISTOPLASMIN SENSITIVITY IN WHITE SCHOOL CHILDREN IN CINCINNATI, 1953*

Years	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	Total
Number tested	270	643	776	727	858	781	763	607	669	463	237	160	140	90	10	7194
Positive to histoplasmin	41	119	170	187	290	283	299	262	333	245	148	93	92	72	8	2642
%	15.2	18.5	21.9	25.7	33.8	36.2	39.2	43.2	49.8	52.9	62.5	58.1	65.7	80.0	80.0	36.7
Positive to blastomycin	12	22	51	61	91	86	111	91	123	88	53	32	28	23	3	875
%	4.4	3.4	6.6	8.4	10.6	11.0	14.5	15.0	18.4	19.0	22.4	20.0	20.0	25.5	30.0	12.2

*Modified from Furcolow *et al.*[1]

TABLE 2

COMPARISON OF SIZE OF COMPONENTS OF PRIMARY COMPLEXES IN HISTOPLASMOSIS AND TUBERCULOSIS*

	1 to 4 mm.	*5 to 10 mm.*	*10 mm. or more*
Primary focus lung			
Histoplasmosis	23	42	2
Tuberculosis	27	7	
Lymph nodes of the primary complex			
Histoplasmosis	48		22
Tuberculosis	21		3

*Modified from Straub and Schwarz.[6]

TABLE 3

X-RAY APPEARANCE OF SPLENIC CALCIFICATION IN AUTOPSY MATERIAL FROM THREE DIFFERENT GEOGRAPHIC AREAS*

Area	*No. of spleens examined*	*"Typical" round foci*		*"Atypical" round foci*	*Vascular shadows*	*Miscellaneous & non-descript*	*Totals*
		No. of Cases	*%*	*No. of Cases*	*No. of Cases*	*No. of Cases*	
Cincinnati	120	53	44	8	4	9	74
Rotterdam	102	2	2	9	4	14	29
New York	108	3	3	5	11	10	29

*Modified from Schwarz *et al.*[5]

at least, severe disseminated disease is much more real than in older-age groups, a situation that is quite analogous to primary tuberculosis. The positive result of blood cultures is not a necessary sign of progressive disease; the number of colonies may be more decisive for prognosis than the fact of a positive culture by itself. Routine blood or bone marrow cultures, like any routine work that is requested without thought and executed without loving care, will seldom give positive results, and these will be even more meager in adults[11] than in children.[12] Positive blood smears are found generally only in terminal dissemination, but positive bone marrow smears can be obtained even in cases with relatively good clinical appearance. The parasitation of phagocytic elements in the blood and bone marrow can assume truly fantastic proportions; we have obtained as many as 7000 colonies from 1 ml. blood, and we have seen

blood clots consisting almost entirely of phagocytic cells with *Histoplasma* in the cytoplasm. Certainly, constitutional factors determine the outcome of the primary infection, and age is one of prime importance. Sex has much less influence than in blastomycosis, where the proportion is 10:1 or more in favor of the male sex. Race has apparently hardly any influence on infection rate and course of the disease.

A point that has not been answered yet is the question of reinfection. Obviously two possibilities of reinfection exist: the endogenous and the exogenous. In the endemic area, with the almost constant exposure to the organism, it should be much easier to acquire an exogenous reinfection than to expect the breakdown of body resistance and dissemination from an arrested primary complex, but probably both forms exist. An example in point is the Cincinnati water tower epidemic, in which a group of twelve men became ill to a greater or lesser degree after cleaning the structure; one must presume that a considerable part of the group had positive skin reactions when they started working, but no factual data exist about this point. Certainly, then, some of those workers had exogenous reinfection; these may have had much milder or subclinical disease.[13]

Comparatively frequently we find, on X-ray examination, circumscribed round lesions that have become known as coin lesions to the radiologist and as tuberculomas to surgeons and pathologists. In the endemic area those fairly well-circumscribed lesions are most frequently caused by *Histoplasma capsulatum*. Another form of chronic pulmonary histoplasmosis is found with increased frequency: cavitary disease. The lesions are indistinguishable on X ray and even on gross specimens from other fungus infections or from tuberculosis.

Extensive tests with eighteen strains of *Histoplasma* collected either in Cincinnati or in different parts of the world, including Africa, reveal that there is great variation in the gross cultural morphology, in the microscopic behavior of the yeast phase, and in the lesions produced by the different strains in animals.[14,15]

We have tested the amino acid requirement of the different strains (unpublished data), and we have come to the conclusion that there is too much variability in different and individual strains at different times and under differing circumstances of humidity, acidity, and temperature to allow this factor to be used for purposes of differentiation between the strains. We have found that the hamster is more susceptible to infection with *Histoplasma capsulatum* than the mouse that is used in most laboratories.[16]

The most valuable single feature dictating the preference of the hamster over the mouse is the fact that most of the former die within 10 to 14 days after injection of massive doses; generally, after a month not a single animal survives. For evaluation of therapeutic experiments this is a most

desirable situation and, as is so frequently the case, the use of a new technical procedure brings rather surprising results. After examining the tissues of the hamsters injected with the American or African strains, we found very astonishing organisms, some enormously large, in the tissue of the hamsters; actual measurements were as great as 27 μ. Some of these organisms stained with hematoxylin uniformly blue, but with special stains such as the Gridley and with the Grocott procedure we could see that layers were formed around the organism. As a matter of fact, it was possible frequently to see in the center the ordinary ovoid yeast form of *H. capsulatum*, 2 or 3 μ in size, often without budding, rarely with budding, surrounded by these layers. We then studied this phenomenon in more detail and found that the size and number of these halos surrounding the yeast cell was an index of time. The halos developed so regularly that we were able to formulate a timetable that tells us almost exactly, if we look at the number and size of these so-called halo cells, how many days after injection an animal died. Very obviously, this is a phenomenon of considerable interest because it is a response specific to the hamster; at least, we have never seen it in mice, rats, guinea pigs, or rabbits.

H. duboisii was described first by English authors[17] from Africa, but special attention was drawn to it by Vanbreuseghem,[18] who observed it in the Belgian Congo. Subsequently several reports by Symmers[19] and others appeared, describing cases in African natives who came to Europe or whose tissues became available. Lately, spontaneous infection with *H. duboisii* has been reported from the Institut Pasteur in Paris, France, as occurring in monkeys imported from equatorial Africa.[20] These organisms are 12 to 20 μ in size and in this respect can hardly be differentiated in tissue, for instance, from those of *Blastomyces dermatitidis*. With increasing experience, we have learned that one never should attempt to make a diagnosis based on the morphologic appearance of organisms alone because of the enormous variety in size that can be observed in tissue.

Obviously, for diagnostic purposes one can sometimes make decisions based on morphology in tissues but, ideally, one should identify any of the yeast cells only after culturing them. *H. duboisii* is frequently less pathogenic to animals than the ordinary American strain, and only if enormous doses are given will an animal succumb to the infection. However, this is not due to lack of tissue reaction. On the contrary, the most fantastic granulomatous lesions are observed after several weeks, and the giant cells, filled with *H. duboisii*, are rather unique in our experience.[21] They are found in mice and hamsters. We do not have much experience with other animals, but occasional reports in the literature of injection of single animals such as guinea pigs indicate that the pathogenicity and tissue reaction are very similar in these animals also. We do

TABLE 4

SIZE OF YEAST CELLS IN TISSUES*

	Average variation/microns	Extremes observed by author
Histoplasma capsulatum	2 to 5	1 to 20
Histoplasma duboisii	10 to 12	2 to 22
Blastomyces dermatitidis	7 to 20	2 to 30
Blastomyces brasiliensis	10 to 30	4 to 80
Candida albicans	3 to 6	1 to 7
Cryptococcus neoformans	5 to 15	3 to 20

*Modified from Schwarz and Baum.[22]

not feel qualified to determine whether *H. duboisii* is a separate organism, a variety, or a subspecies; frankly we do not consider this a major problem. *H. duboisii* is an organism that can be differentiated morphologically to some degree in the yeast phase (the mycelial phase is undistinguishable) due to the presence after a week or two of innumerable large forms 10 μ or more in size. While we have shown that variable numbers of large cells occur also in American strains, the number is never so impressive as in *H. duboisii* cultures, and even the so-called large forms of *H. capsulatum* are generally below 10 μ and only exceptionally above that size. We then must conclude that *H. capsulatum* in the yeast phase occurs in several different forms:

(1) The classic type, which is about 2 to 5 μ in size.

(2) The large form of the American strain, which is 6 to 10 μ and is generally found in older cultures kept in the yeast phase and sometimes in tissues, especially in necrotic or caseated lesions.

(3) The artifically induced large forms of *Histoplasma*, which we observed in tissue explants. The technique for producing these explants is extremely simple. We take tissue either from animals that have been inoculated several weeks previously with *H. capsulatum* or, if available, from human sources with proved disseminated histoplasmosis, and cut small pieces that are put on the surfaces of blood-agar plates. These pieces are then incubated in a moist chamber for periods of time ranging from 1 day to 3 to 4 weeks at 37° C. Within a few days, large cells develop; these apparently are due to inhibition of budding, which explains the accumulation of cytoplasm.

(4) The peculiar forms that we call the halo cells, which develop, apparently, only in hamster tissue with American strains. Similar forms have been observed in spontaneous disease of humans and monkeys infected with *H. duboisii*.

(5) The "*duboisii* form" that is produced in tissues, frequently consisting of a mixture of small and large cells. In time, however, at least in the experimental animal, fewer small forms and more large ones are seen, the large measuring between 10 and 20 μ and, as a rule, showing rather round or slightly oval forms that produce peculiar enormous giant cells in the tissues of hamsters and mice. We have mentioned repeatedly that there is great variation to be seen in the yeast phase of the biphasic organisms; certainly anyone who has had any experience with human material in blastomycosis, histoplasmosis, or sporotrichosis will agree with this statement. We have seen in North American blastomycosis, for instance, organisms as small as 2 μ and as large as 30 μ. It is quite obvious, then, that culture of them is highly desirable, especially since combinations of the diseases have been described and must be expected.

Summary

Histoplasmosis originating in the lung is caused by inhalation of spore-containing dust.

A primary complex is formed, usually larger than the one seen in tuberculosis; dissemination during the primary infection is common, and results are evident in the form of disseminated calcific lesions, especially in the spleen.

The great variation in the morphology of the organism in tissues makes cultural proof highly desirable. The African variety described as *Histoplasma duboisii* can be differentiated from the "classic" strains of *H. capsulatum* by its size, shape, and behavior in experimental animals.

Giant forms of *Histoplasma* induced on tissue explants and in infected hamsters are mentioned, and five morphologically different forms of yeasts are enumerated.

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CONFERENCES HELD

SECTION OF PSYCHOLOGY

CONCEPTUAL AND METHODOLOGICAL PROBLEMS IN PSYCHOANALYSIS

Tuesday, March 4 and
Wednesday, March 5, 1958

Conference Chairman
Leopold Bellak

Altro Health and Rehabilitation Services
New York, N. Y.

TUESDAY, MARCH 4, 1958

AN ATTEMPT AT THE SYSTEMATIC RESTATEMENT OF THE LIBIDO THEORY

Session Chairman: Alfred H. Stanton
McLean Hospital of Massachusetts General Hospital and
Harvard University Medical School, Boston, Mass.

9:00 A.M.

Greetings from the Academy – Elaine R. Grimm, Chairman, Section of Psychology, The New York Academy of Sciences, New York, N. Y.; The New York Hospital, New York, N. Y.

Introductory Remarks – Leopold Bellak, Altro Health and Rehabilitation Services, New York, N. Y.

"Propositions Concerning Libidinal Zones: Libidinal Aims and Modes of Gratification" – Thomas S. Szasz, State University of New York College of Medicine, Upstate Medical Center, Syracuse, N. Y.

Discussion Leader: Margaret Brenman, Austin Riggs Foundation, Stockbridge, Mass.

Discussion Panel: Leopold Bellak, Margaret Brenman, Rudolph Ekstein, Lawrence S. Kubie, Nathan Leites, I. Arthur Mirsky, Mortimer Ostow, Eugene Pumpian-Mindlin, Nevitt Sanford, Thomas S. Szasz, and Martha Wolfenstein.

Session Chairman: I. Arthur Mirsky
University of Pittsburgh Medical School, Pittsburgh, Pa.

2:00 P.M.
"Propositions Concerning Object Choices" – Alfred H. Stanton, McLean Hospital of Massachusetts General Hospital and Harvard University Medical School, Boston, Mass.

Discussion Leader: Martha Wolfenstein, Graduate School of Psychology, College of the City of New York, New York, N. Y.

Discussion Panel: Leopold Bellak, Margaret Brenman, Rudolph Ekstein, Lawrence S. Kubie, Nathan Leites, I. Arthur Mirsky, Mortimer Ostow, Eugene Pumpian-Mindlin, Nevitt Sanford, Thomas S. Szasz, and Martha Wolfenstein.

Session Chairman: Lawrence S. Kubie
Columbia University, New York, N. Y., and Yale University, New Haven, Conn.

8:30 P.M.
"Propositions Concerning Energetic-Economic Aspects of Libido Theory" – Eugene Pumpian-Mindlin, Los Angeles, Calif.

Discussion Leader: Nevitt Sanford, Vassar College, Poughkeepsie, N. Y., and University of California, Berkeley, Calif.

Discussion Panel: Leopold Bellak, Margaret Brenman, Rudolph Ekstein, Lawrence S. Kubie, Nathan Leites, I. Arthur Mirsky, Mortimer Ostow, Eugene Pumpian-Mindlin, Nevitt Sanford, Thomas S. Szasz, and Martha Wolfenstein.

WEDNESDAY, MARCH 5, 1958

AN ATTEMPT AT THE SYSTEMATIC RESTATEMENT OF SOME BASIC PSYCHOANALYTIC CONSTRUCTS

Session Chairman: Thomas S. Szasz
State University of New York College of Medicine, Upstate Medical Center Syracuse, N. Y.

9:30 A.M.
"The Unconscious" – Leopold Bellak, Altro Health and Rehabilitation Services, New York, N. Y.

Discussion Leader: Nevitt Sanford, Vassar College, Poughkeepsie, N. Y., and University of California, Berkeley, Calif.

Discussion Panel: Leopold Bellak, Margaret Brenman, Rudolph Ekstein, Lawrence S. Kubie, Nathan Leites, I. Arthur Mirsky, Mortimer Ostow, Eugene Pumpian-Mindlin, Nevitt Sanford, Thomas S. Szasz, and Martha Wolfenstein.

Session Chairman: Mortimer Ostow
Riverdale, N. Y.

2:00 P.M.
"The Preconscious"—I. Arthur Mirsky, University of Pittsburgh Medical School, Pittsburgh, Pa.

Discussion Leader: Nathan Leites, Yale University, New Haven, Conn.

Discussion Panel: Leopold Bellak, Margaret Brenman, Rudolph Ekstein, Lawrence S. Kubie, Nathan Leites, I. Arthur Mirsky, Mortimer Ostow, Eugene Pumpian-Mindlin, Nevitt Sanford, Thomas S. Szasz, and Martha Wolfenstein.

Session Chairman: Eugene Pumpian-Mindlin
Los Angeles, Calif.

8:30 P.M.
"The Structural Model: Ego, Id, and Superego" – Mortimer Ostow, Riverdale, N. Y.

Discussion Leader: Rudolph Ekstein, Reiss-Davis Clinic, Los Angeles, Calif.

Discussion Panel: Leopold Bellak, Margaret Brenman, Rudolph Ekstein, Lawrence S. Kubie, Nathan Leites, I. Arthur Mirsky, Mortimer Ostow, Eugene Pumpian-Mindlin, Nevitt Sanford, Thomas S. Szasz, and Martha Wolfenstein.

SECTION OF BIOLOGY

SURGICAL CONVALESCENCE

Friday, March 7 and
Saturday, March 8, 1958

Conference Co-Chairmen
F. C. Dohan,
Assistant Professor of Clinical Medicine
School of Medicine, University of Pennsylvania, Philadelphia, Pa.

N. Henry Moss
Instructor in Surgery
School of Medicine, University of Pennsylvania, Philadelphia, Pa.

Cancer Coordinator and Director of Tumor Board,
Albert Einstein Medical Center, Philadelphia, Pa.

FRIDAY, MARCH 7, 1958

Session Chairman: I. S. Ravdin
John Rhea Barton Professor of Surgery
School of Medicine, University of Pennsylvania, Philadelphia, Pa.

9:00 A.M. –
Greetings from the Academy – Emerson Day, Recording Secretary, The New York Academy of Sciences, New York, N. Y.; Memorial Center for Cancer and Allied Diseases, New York, N. Y.

"Introduction: Problems and Advances in Surgical Convalescence" – I. S. Ravdin, John Rhea Barton Professor of Surgery, School of Medicine, University of Pennsylvania, Philadelphia, Pa.

"Phases of Surgical Convalescence and the Metabolic Response to Surgery" – Francis D. Moore, Moseley Professor of Surgery, Harvard Medical School, Boston, Mass.

"The Metabolic Response to Surgery: Systemic-Adrenal-Compositional" – James D. Hardy, Professor of Surgery, Medical Center, University of Mississippi, University, Miss.

Discussion

"Wound Healing" – J. Englebert Dunphy, Professor of Surgery, Harvard Medical School, Boston, Mass.

"A Survey of Some Physiological Responses of Domestic Animals During the Immediate Postsurgical Period" – Mark W. Allam, Professor of Vet-

erinary Surgery and Dean, School of Veterinary Medicine, University of Pennsylvania, Philadelphia, Pa.

Session Chairman: Paul Hawley
The Director, American College of Surgeons, Chicago, Ill.

2:00 P.M. –
"The Duration of Surgical Convalescence as Indicated by Insurance Statistics" – Harvey J. Saffeir, Assistant Actuary, The Travelers Insurance Company, Hartford, Conn.

"Surgical Convalescence: When Does It End?" – F. C. Dohan, Assistant Professor of Clinical Medicine, School of Medicine, University of Pennsylvania, Philadelphia, Pa., and N. Henry Moss, Instructor in Surgery, School of Medicine, University of Pennsylvania, Philadelphia, Pa., also Cancer Coordinator and Director of Tumor Board, Albert Einstein Medical Center, Philadelphia, Pa.

Discussion

"Criteria for Fitness and Comments on Negative Nitrogen Balance" – Henry P. Taylor, Professor of Physiological Hygiene, and Ancel Keys, Professor of Physiological Hygiene, University of Minnesota, Minneapolis, Minn.

"Convalescence, Rehabilitation, and the Therapeutic Use of Stress" – Howard Rusk, Professor and Chairman, Department of Physical Medicine and Rehabilitation, New York University College of Medicine, New York, N. Y.; Director, Institute of Physical Medicine of the New York University-Bellevue Medical Center, New York, N. Y.

Discussion

"Sociological Importance of the Convalescent Period" and Summary of Afternoon Session – Paul Hawley, The Director, American College of Surgeons, Chicago, Ill.

SATURDAY, MARCH 8, 1958

Session Chairman: Frank B. Berry
Assistant Secretary of Defense (Health and Medical)
Washington, D. C.

9:00 A.M. –
"The Importance of Convalescence and Rehabilitation to National Manpower" – Frank B. Berry, Assistant Secretary of Defense (Health and Medical), Washington, D.C.

"Psychological Factors in Surgical Convalescence" – Arthur M. Sutherland, Attending Physician (Psychiatry), Memorial Center for Cancer and Allied Diseases, New York, N. Y.; Associate Professor of Clinical Medicine, Cornell University Medical School, New York, N. Y.

"A Study of the Complications of the Posthospitalization Period of Surgical Convalescence" – Charles Johnston, Professor of Surgery, and Kenneth Bartels, Assistant Instructor in Surgery, Wayne State University, Detroit, Mich.

"Return to Work After Surgery: An Industrial Study" – Allan J. Fleming, Medical Director, E. I. du Pont de Nemours and Company, Wilmington, Del.

Discussion

"Early Ambulation and Return to 'Full Duty' in the U. S. Air Force" – David Gold, Colonel USAF (MC), Chief, Consultants Group, Office of the Surgeon General, Department of the Air Force, Washington, D. C.

"Early Ambulation: Its Influence on Postoperative Complications and Return to Work Following Hernioplasty, in a Rural Population" – John H. Powers, Surgeon-in-Chief, Mary Imogene Bassett Hospital, Cooperstown, N. Y.; Clinical Professor of Surgery, College of Physicians and Surgeons, Columbia University, New York, N. Y.

Discussion

Summary – F. C. Dohan, Assistant Professor of Clinical Medicine, School of Medicine, University of Pennsylvania, Philadelphia, Pa.

SCREENING PROCEDURES FOR EXPERIMENTAL CANCER CHEMOTHERAPY

Thursday, March 13,
Friday, March 14, and
Saturday, March 15, 1958

Conference Chairman: C. Chester Stock
Sloan-Kettering Institute for Cancer Research, New York, N. Y.

THURSDAY, MARCH 13, 1958

MICROBIOLOGICAL STUDIES

Session Chairman: Esmond E. Snell
University of California, Berkeley, Calif.

9:00 A.M. –

Greetings from the Academy – A. S. Gordon, Chairman, Section of Biology, The New York Academy of Sciences, New York, N. Y.; New York University, New York, N. Y.

Introduction to Conference – Kenneth Endicott, Cancer Chemotherapy National Service Center, Bethesda, Md.

Introductory Remarks – Esmond E. Snell, University of California, Berkeley, Calif.

"A Comparative Study of Use of Microorganisms in Screening of Antitumor Agents" – G. E. Foley, Children's Cancer Research Foundation, Inc., Boston, Mass.

Discussants: George W. Kidder, Amherst College, Amherst, Mass.
Philip S. Thayer, Arthur D. Little, Inc., Cambridge, Mass.

"Utility of Drug-Resistant Organisms in Cancer Chemotherapy Studies" – Frank Schabel, Jr., Southern Research Institute, Birmingham, Ala.

Discussants: Dorris Hutchison, Sloan-Kettering Institute for Cancer Research, New York, N. Y.

"General Considerations in Use of Microorganisms in Screening Antitumor Agents" – S. H. Hutner, Helene A. Nathan, S. Aaronson, and H. Baker, Haskins Laboratories, New York, N. Y.

"Special Microbiological Systems":

(a) "Anaerobic Organisms" – William T. Bradner, Sloan-Kettering Institute for Cancer Research, New York, N. Y.

(b) "Observations on Mutagenicity" – Waclaw Szybalski, Institute of Microbiology, Rutgers University, New Brunswick, N. J.

(c) "For Purine and Pyrimidine Antagonism" – George H. Hitchings, The Wellcome Research Laboratories, Tuckahoe, N. Y.

NORMAL AND MALIGNANT CELLS IN TISSUE CULTURE

Session Chairman: Harry Eagle

National Institute of Allergy and Infectious Diseases, Public Health Service, Bethesda, Md.

2:00 P.M. –

"Tumorigenic Activity of Cultures" – Alice Moore, Sloan-Kettering Institute for Cancer Research, New York, N. Y., and G. E. Foley, Children's Cancer Research Foundation, Inc., Boston, Mass.

"Comparison of Biological Qualities of 'Transformed' Cells of Normal and Cancerous Origin" – Joseph Leighton and Richard L. Kalla, University of Pittsburgh School of Medicine, Pittsburgh, Pa.

"Tumor-Specific Cytotoxicity of Antitumor Agents" – John J. Biesele, Sloan-Kettering Institute for Cancer Research, New York, N. Y.

"Biochemical Properties and Susceptibility to Antitumor Agents" – Harry Eagle, National Institute of Allergy and Infectious Diseases, Public Health Service, Bethesda, Md.

Discussants: Irving S. Johnson, The Lilly Research Laboratories, Eli Lilly and Company, Indianapolis, Ind.
George O. Gey, Finney-Howell Cancer Research Laboratory, The Johns Hopkins Hospital, Baltimore, Md.
Charles G. Smith, The Upjohn Company, Kalamazoo, Mich.
Lawrence Berman, College of Medicine, Wayne State University, Detroit, Mich.

Open Discussion

ASCITES TUMORS

Session Chairman: Theodore S. Hauschka

Roswell Park Memorial Institute, Buffalo, N. Y.

3:50 P.M. –

"Assay of Mitotic Poisons on the Hyperdiploid Ehrlich Ascites Carcinoma" – Hans Lettré, Institut für Experimentelle Krebsforschung der Universität Heidelberg, Heidelberg, Germany.

"Parameters of Ascites Tumor Growth" – Harvey M. Patt, Argonne National Laboratory, Chicago, Ill.

"Relative Sensitivity of the Solid and Ascites Forms of Sarcoma 180 to Inhibitory Compounds" – Kanematsu Sugiura, Sloan-Kettering Institute for Cancer Research, New York, N. Y.

"Experiences Gained in the Testing of Growth Inhibitors Against Anaplastic PRI Mouse Ascites Tumor" – George Tarnowski, Sloan-Kettering Institute for Cancer Research, New York, N. Y., and Irwin Bross, Cornell University Medical College, New York, N. Y.; Sloan-Kettering Institute for Cancer Research, New York, N. Y.

"Total Packed Cell Volume (Ascitocrit) as a Measure of Growth Retardation in Four Mouse Ascites Tumors" – E. N. Sassenrath, School of Medicine, University of Califomia, Berkeley, Calif.

"Screening with Ascites Hepatomas" – Tomizo Yoshida, Tokyo University Medical College, Tokyo, Japan.

FRIDAY, MARCH 14, 1958

LYMPHOMAS

Session Chairman: Lloyd W. Law

National Cancer Institute, Public Health Service, Bethesda, Md.

9:00 A.M. –

"Morphology and Pathology of Mouse Lymphomas – Relationship to Man" – Thelma Dunn, National Cancer Institute, Public Health Service, Bethesda, Md.

"Biology and Responsiveness of Lymphomas" – Michael Potter, National Cancer Institute, Public Health Service, Bethesda, Md.

"Lymphomas in Other Rodents and Responsiveness to Drugs" – Wilhelmina F. Dunning, University of Miami, Coral Gables, Fla.

Discussant: Ralph Jones, Jr., University of Miami School of Medicine, Jackson Memorial Hospital, Miami, Fla.

"Cell Culture of Leukemias and Drug Responsiveness" – Glenn A. Fischer, Yale University School of Medicine, New Haven, Conn.

HORMONE-RESPONSIVE TUMORS

Session Chairman: George W. Woolley

Sloan-Kettering Institute for Cancer Research, New York, N. Y.

10:20 A.M. –

"Screening Techniques and the Problem of Hormone-Responsive Tumors" Jacob Furth, Children's Cancer Research Foundation, Boston, Mass.

Discussants: Philip C. Merker, Sloan-Kettering Institute for Cancer Research, New York, N. Y.
R. L. Noble, The University of Western Ontario, London, Ont., Canada.
Wilhelmina F. Dunning, University of Miami, Coral Gables, Fla.
Vincent Hollander, University of Virginia, Charlottesville, Va.

"Hormonal Patterns as Indices of Tumor Activity" – Ralph I. Dorfman, Worcester Foundation, Worcester, Mass.

"Screening Hormonal Agents for Anticancer Activity in Man" – Albert Segaloff, Ochsner Medical Foundation, New Orleans, La.

"Screening Steroids Against a Spectrum of Tumors" – C. Chester Stock and Kanematsu Sugiura, Sloan-Kettering Institute for Cancer Research, New York, N. Y.

"Methods of Evaluation of Steroids on Human Tumors Growing on the Chick Embryo" – John J. Harris, Sloan-Kettering Institute for Cancer Research, New York, N. Y.

THE HUMAN TUMOR IN HETEROLOGOUS HOSTS

Session Chairman: J. Walter Wilson
Brown University, Providence, R. I.

2:00 P.M. –
"The Biology of the Human Tumor in the Heterologous Host" – Helene Toolan, Sloan-Kettering Institute for Cancer Research, New York, N. Y.

"The Human Tumor in Cancer Chemotherapy in the Conditioned Rat" – Morris N. Teller, Philip C. Merker, Joy E. Palm, and George W. Woolley, Sloan-Kettering Institute for Cancer Research and Sloan-Kettering Division, Cornell University Medical College, New York, N. Y.

"Observations on Chemotherapy of Human Tumors (HSI and HEP-3) in the Heterologous Host" – William S. Marsh and M. R. Cullen, John L. Smith Memorial for Cancer Research, Chas. Pfizer and Co., Inc., Maywood, N. J.

Discussants: Philip Merker, Sloan-Kettering Institute for Cancer Research, New York, N. Y.
Wilhelmina F. Dunning, University of Miami, Coral Gables, Fla.

"The Human Tumor Grown in the Egg" – John J. Harris, Sloan-Kettering Institute for Cancer Research, New York, N. Y.

Discussant: Charles P. Dagg, Brown University, Providence, R. I.

"The Human Tumor in the Hamster" – Alfred H. Handler, Children's Cancer Research Foundation, Inc., Boston, Mass.

Discussant: Sheldon C. Sommers, Massachusetts Memorial Hospitals, Boston, Mass.

"The Human Tumor in the Mouse" – Ruth Gallily, University of Illinois, Urbana, Ill.; Sloan-Kettering Institute for Cancer Research, New York, N. Y.

Discussant: Shozo Takayama, Sloan-Kettering Institute for Cancer Research, New York, N. Y.

"Host-Conditioning Problems" – Joy E. Palm, Morris N. Teller, Philip C. Merker, and George W. Woolley, Sloan-Kettering Institute for Cancer Research and Sloan-Kettering Division, Cornell University Medical College, New York, N. Y.

Discussant: George W. Woolley, Sloan-Kettering Institute for Cancer Research, New York, N. Y.

SPECIAL SYSTEMS

Session Chairman: Murray J. Shear

National Cancer Institute, Public Health Service, Bethesda, Md.

8:00 P.M. –

"Resistant Leukemias and Tumors" – Joseph H. Burchenal, Sloan-Kettering Institute for Cancer Research, New York, N. Y.

Discussants: Lloyd W. Law, National Cancer Institute, Public Health Service, Bethesda, Md.
Frank M. Schabel, Jr., Southern Research Institute, Birmingham, Ala.
Dorris Hutchison, Sloan-Kettering Institute for Cancer Research, New York, N. Y.

"Characterization of the Specificity of Action of Tumor-Inhibiting Compounds" – Prosper Loustalot, P. A. Desaulles, and R. Meier, CIBA Ltd., Basel, Switzerland.

"Trial of Chemotherapeutic Agents in Spontaneous Tumors in Dogs" – John R. McCoy, Bureau of Biological Research, Rutgers University, New Brunswick, N. J.

"Practical and Theoretical Considerations in the Use of Induced and Spontaneous Tumors in Cancer Chemotherapy" – Jean Scholler, Sloan-Kettering Institute for Cancer Research, New York, N. Y.

"Possible Utility of the Rous Sarcoma for Antitumor Screening" – Irving S. Johnson, Linville A. Baker, and Howard F. Wright, The Lilly Research Laboratories, Eli Lilly and Company, Indianapolis, Ind.

Discussants: Vincent Groupe, Institute of Microbiology, Rutgers University, New Brunswick, N. J.
W. Ray Bryan, National Cancer Institute, Public Health Service, Bethesda, Md.

"The Use of Ascites in Solid Agar for Cancer Chemotherapy Screening" Joseph A. Di Paolo and George E. Moore, Roswell Park Memorial Institute, Buffalo, N. Y.

"Role of Chance Observations in Chemotherapy: Vinca Rosea" – C. T. Beer, J. H. Cutts, and R. L. Noble, The University of Western Ontario, London, Ont., Canada.

SATURDAY, MARCH 15, 1958

CONSIDERATIONS IN EXPERIMENTAL CANCER CHEMOTHERAPY

Session Chairman: John T. Litchfield
Lederle Laboratories Division, American Cyanamid Company
Pearl River, N. Y.

9:00 A.M. –

"Biometric Considerations in Experimental Design for Large-Scale Anticancer Screening" – Marvin Schneiderman, Cancer Chemotherapy National Service Center, Bethesda, Md., and Peter Armitage, British Medical Research Council, and Cancer Chemotherapy National Service Center, Bethesda, Md.

Discussants: Frank M. Schabel, Jr., Southern Research Institute, Birmingham, Ala.
Charles W. Dunnett, Lederle Laboratories Division, American Cyanamid Company, Pearl River, N. Y.
Irwin Bross, Department of Public Health and Preventive Medicine, Cornell University Medical College; Sloan-Kettering Institute for Cancer Research, New York, N. Y.
Charles M. Mottley, John L. Smith Memorial for Cancer Research, Chas. Pfizer and Co., Inc., Maywood, N. J.

"Combination Chemotherapy" – Nathan Mantel, National Cancer Institute, Public Health Service, Bethesda, Md.

Discussants: Donald A. Clarke, Sloan-Kettering Institute for Cancer Research, New York, N. Y.
C. E. P. Box, Princeton University, Princeton, N. J.

Frank M. Schabel, Jr., Southern Research Institute, Birmingham, Ala.
Daniel M. Shapiro, School of Medicine, University of Miami, Miami, Fla.

"Extrapolation to Studies in Man" – Louis Lasagna, Johns Hopkins Hospital, Baltimore, Md.

Discussants: Alfred Gellhorn, Francis Delafield Hospital, New York, N. Y.
Joseph Leiter, Cancer Chemotherapy National Service Center, Bethesda, Md.
Frederick S. Philips, Sloan-Kettering Institute for Cancer Research, New York, N. Y.
Donald A. Clarke, Sloan-Kettering Institute for Cancer Research, New York, N. Y.
Frank M. Schabel, Jr., Southern Research Institute, Birmingham, Ala.

Summary of Session – John T. Litchfield, Lederle Laboratories Division, American Cyanamid Company, Pearl River, N. Y.

Summary of Conference – C. Chester Stock, Sloan-Kettering Institute for Cancer Research, New York, N. Y.

ORGANIZING COMMITTEE

C. Chester Stock, *Chairman*

Harry Eagle	Joseph Leiter
Karl Folkers	Harold P. Rusch
Theodore Hauschka	Esmond E. Snell
James F. Holland	J. Walter Wilson
Lloyd W. Law	George W. Woolley

METABOLIC FACTORS IN CARDIAC CONTRACTILITY

Tuesday, March 18 and
Wednesday, March 19, 1958

Conference Co-Chairmen
Menard M. Gertler
Institute of Physical Medicine and Rehabilitation, New York University-Bellevue Medical Center, New York, N. Y.
G. W. E. Plaut
New York University College of Medicine, New York, N. Y.

TUESDAY, MARCH 18, 1958

IONS AND THEIR RELATIONSHIP TO HEART MUSCLE

Ions that are known to influence cardiac action will be discussed with the view of considering the enzyme systems that may be affected by a deviation in their concentration. In addition, the possible effects on cardiac metabolism or action resultant from a variation of these factors will be considered.

Session Chairman: L. Corsan Reid
New York University College of Medicine, New York, N. Y.

9:00 A.M.

Greetings from the Academy – A. S. Gordon, Chairman, Section of Biology, The New York Academy of Sciences, New York, N. Y.; New York University, New York, N. Y.

Introduction to the Conference – Howard A. Rusk, New York University College of Medicine, New York, N. Y.

"General Survey of Ion Concentration" – George C. Griffith, University of Southern California School of Medicine, Los Angeles, Calif.

"Maintenance of Concentration Gradients and Regulation of Cell Volume" – Alexander Leaf, Massachusetts General Hospital, Boston, Mass.

"Physiological Effect of Ions" – Richard Overman. University of Tennessee Medical School, Memphis, Tenn.

"Effect of Ions on Enzyme Activity" – John A. Muntz, The Albany Medical College of Union University, Albany, N. Y.

ENERGY-GENERATING SYSTEMS: GLUCOSE UTILIZATION

The metabolism of glucose will be considered under various aspects, such as enzyme systems involved, Embden-Meyerhof scheme, shunt mechanisms, and glycogen storage and utilization. The effect of disturbances of these processes and their possible effects, for example, on the myocardium and contractility, will be considered.

Session Chairman: J. Murray Steele
New York University College of Medicine, New York, N. Y. and Goldwater Memorial Hospital, New York, N. Y.

2:00 P.M.

"Glycolysis and Oxidative Pathways" – E. Racker, The Public Health Research Institute of the City of New York, New York, N. Y.

"Glycogen Storage and Utilization" – Paul A. di Sant'Agnese, Columbia-Presbyterian Medical Center, New York, N. Y.

"Oxidative Decarboxylation Reactions" – I. C. Gunsalus, University of Illinois, Urbana, Ill.

"The Role of CO_2 Fixation in Carbohydrate Utilization and Synthesis" – Merton F. Utter, Western Reserve University Medical School, Cleveland, Ohio.

Discussion Leader: Santiago Grisolia, University of Kansas Medical School, Kansas City, Kan.

WEDNESDAY, MARCH 19, 1958

ENERGY-TRANSFER SYSTEMS AND THEIR RELATIONSHIP TO THE NORMAL AND THE FAILING HEART

The role of vitamins, coenzymes, and cofactors will be considered. The energy-yielding role of the electron transport chain will be discussed, as will energy-transference mechanisms. The use of inhibitors of the electron transport chain will be considered and their possible value in therapeutics. The high-energy phosphate bond will be considered as a source of energy for the normal heart, and the possible implications for the failing heart will be evaluated.

Session Chairman: F. Lipmann
The Rockefeller Institute for Medical Research, New York, N. Y.

9:15 A.M.

"Conservation of Energy in Cardiac Muscle" – Robert E. Olson, University of Pittsburgh Graduate School of Public Health, Pittsburgh, Pa.

"Electron Transport in Cardiac Tissue" – Thomas P. Singer, Institute for Medical Research, Henry Ford Hospital, Detroit, Mich.

"Oxidative Phosphorylation and Possible Mechanism of Regulation" – P. Siekevitz, The Rockefeller Institute for Medical Research, New York, N. Y.

Discussion Leader: G. W. E. Plaut, New York University College of Medicine, New York, N. Y.

ENERGY-UTILIZING SYSTEMS

The contractile elements such as myosin, actomyosin, and visible structural elements of the myocardium will be discussed. Evidence of drug action will also be considered.

Session Chairman: L. W. Eichna
New York University College of Medicine, New York, N. Y.

2:00 P.M.

"Microscopical Organization of Cardiac Muscle Fibers" – George Palade, The Rockefeller Institute for Medical Research, New York, N. Y.

"Chemical Thermodynamics of Contraction" – R. J. Podolsky, Naval Medical Research Institute, Bethesda, Md.

"Muscle Proteins and Energy Utilization" – John Gergeley, Massachusetts General Hospital, Boston, Mass.

"Efficiency of Each Ventricle" – Richard J. Bing, Washington University School of Medicine, St. Louis, Mo.

Concluding Remarks – Menard M. Gertler, Institute of Physical Medicine and Rehabilitation, New York University-Bellevue Medical Center, New York, N. Y.

THE BIOLOGY OF THE AMOEBA

Monday, March 24 and
Tuesday, March 25, 1958

Conference Chairman: H. I. Hirshfield
Graduate School of Arts and Science, New York University
New York, N. Y.

MONDAY, MARCH 24, 1958

STRUCTURAL AND TAXONOMIC CONSIDERATIONS

Session Chairman: H. W. Chalkley
National Cancer Institute, Public Health Service, Bethesda, Md.

9:00 A.M. –
Greetings from the Academy – M. J. Kopac, Vice-President, The New York Academy of Sciences, New York, N. Y.; New York University, New York, N. Y.

"The Conference and Its Aims" – H. I. Hirshfield, Graduate School of Arts and Science, New York University, New York, N. Y.

"Cytoplasmic Inclusions of Pelomyxa" – R. Torch, University of Vermont, Burlington, Vt.

"Particulates of Amoeba" – R. Kassel, St. Vincent's Hospital, New York, N. Y.

"Electron Microscope Studies on Amoeba" – G. Pappas, College of Physicians and Surgeons, Columbia University, New York, N. Y.

"Pelomyxa and Related Organisms" – R. R. Kudo, Institute of Microbiology, Rutgers University, New Brunswick, N. J.

PHYSICAL STUDIES AND CELL DIVISION

Session Chairman: D. A. Marsland
New York University, New York, N. Y.

2:00 P.M. –
"Sol-Gel Transformations in Amoeba" – J. Landau, U. S. Veterans Administration Hospital, Albany, N. Y.

"Synchronization of Cell Division in Amoeba" – T. James, University of California at Los Angeles, Los Angeles, Calif.

"Pinocytosis in Pelomyxa" – J. M. Marshall, School of Medicine, University of Pennsylvania, Philadelphia, Pa.

"Problems of Pinocytosis, With Special Regard to Amoebae" – Heinz Holter, Carlsberg Laboratory, Copenhagen, Denmark.

8:00 P.M. –

"Research on Amoeba in 2158 A.D." – M. J. Kopac, New York University, New York, N. Y.

TUESDAY, MARCH 25, 1958

CYTOCHEMISTRY AND ENZYMES

Session Chairman: M. J. Kopac
New York University, New York, N. Y.

9:30 A.M. –

"Microscopic Enzyme Chemistry in Amoeba" – H. Guthwin, College of Physicians and Surgeons, Columbia University, New York, N. Y.

"Cytochemical Differentiation in Amoeba" – I. M. Heller, New York University-Bellevue Medical Center, New York, N. Y.

"Physiological and Morphological Observations on Amoeba" – Adolph I. Cohen, Washington University School of Medicine, St. Louis, Mo.

"Tracer Studies in Amoeba" – W. Plaut, University of Wisconsin, Madison, Wis.

"Effects of Selected Chemical Agents on Amoeba" – A. M. Zimmerman, University of California, Berkeley, Calif.

NUCLEAR-CYTOPLASMIC RELATIONSHIPS

Session Chairman: Heinz Holter
Carlsberg Laboratory, Copenhagen, Denmark

2:00 P.M. –

"Nuclear Control of Cytoplasmic Activities" – H. I. Hirshfield, Graduate School of Arts and Science, New York University, New York, N. Y.

"Microtechniques in Amoeba Studies" – D. Prescott, University of California Medical Center, Los Angeles, Calif. (To be read by title.)

"Micrurgical Studies on Pelomyxa" – E. E. Daniels, Argonne National Laboratory, Lemont, Ill.

"Nuclear Transplantations in Amoeba" – J. Danielli, King's College, University of London, London, England.

"Cytoplasmic Dependence in Amoeba" – J. Brachet, Université Libre de Bruxelles, Brussels, Belgium. (To be read by title.)

NEW MEMBERS

NEW MEMBERS

Elected March 27, 1958

SUSTAINING MEMBERSHIP

Barrett, Bernard Morris, M.D., Medicine & Surgery. Ear, Nose, Throat Specialist, Pensacola, Fla.

Carrington, George Lunsford, M.D., General Surgery. Surgeon, Alamance General Hospital, Burlington, N. C.

Crume, Thomas J., M.D., Internal Medicine, Practicing Physician, Owensboro, Ky.

Finch, R. B., M.D., Biology. Owner, West Covina Medical Center, West Covina, Calif.

Keasbey, Anthony Quinton, M.A., Fossil Fishes. Instructor in Biology, Riverdale Country School, New York, N. Y.

McColloch, Robert J., Ph.D., Physical Chemistry. Associate Professor, University of Wyoming, Laramie, Wyo.

Pomerance, William, M.D., Obstetrics. Attending Gynecologist & Obstetrician. Jewish Hospital, Brooklyn, N. Y.

Redman, Pierre, M.D., Leukemia & Cancer Research. Active Practice, Mena, Ark.

Sacks, Harry J., M.D., Pathology, Associate Director, Division of Laboratories, Cedars of Lebanon Hospital, Los Angeles, Calif.

Schwartz, Louis Adrian, M.D., Psychiatry. Associate Professor, Wayne State University Medical School, Detroit, Mich.

Snay, Hans Günter, Dr. Ing., Hydrodynamics. Senior Research Scientist, U. S. Naval Ordnance Laboratory, Silver Spring, Md.

Strickland, Robert Lee, M.D., Medicine. Private Practice, Oregon City, Oreg.

ACTIVE MEMBERSHIP

Aaronson, Bernard S., Ph.D., Psychology. Director, Department of Psychology, Trenton State Hospital, Trenton, N. J.

Abramson, Daniel, M.D., Obstetrics & Gynecology. Assistant Clinical Professor, Obstetrics, Harvard Medical School, Cambridge, Mass.

Adelson, Marvin, Ph.D., Psychology. Head, Operations & Equipment Analysis Section, Hughes Aircraft Company, Fullerton, Calif.

Affleck, Dean Craig, Ph.D., Psychodiagnostic Testing. Assistant Professor, Medical Psychology, University of Nebraska College of Medicine, Omaha, Nebr.

Albizu-Miranda, Carlos, Ph.D., Psychology. Associate Professor, University of Puerto Rico, Rio Piedras, Puerto Rico.

Alexander, William A., Jr., Ph.D., Clinical Psychology. Chief Psychologist, Hawthornden State Hospital, Macedonia, Ohio.

Anderson, Charles J., M.S., Chemistry. Project Leader, Vitro Corporation, Little Falls, N. J.

Anderson, LeMoyne W., M.S., Library Science. Director of Libraries, Colorado State University, Fort Collins, Colo.

Arce Q., Alejandro, M.D., Hematology. Research Associate, St. Elizabeth's Hospital, Cambridge, Mass.

Archer, Wesley L., Ph.D., Organic Chemistry. Research & Development Chemist, Dow Chemical Company, Midland, Mich.

Arsenian, John, Ph.D., Schizophrenia. Director, Psychological Research, Boston State Hospital, Boston, Mass.

Aust, J. Bradley, Ph.D., Surgery. Instructor, University of Minnesota, Minneapolis, Minn.

Bacila, Metry, M.D., Biochemistry. Chief, Division of Experimental Pathology, University of Pennsylvania School of Medicine, Philadelphia, Pa.

Bader, Richard G., Ph.D., Oceanography. Associate Professor, A. & M. College of Texas, College Station, Texas.

Bagby, James W., Ph.D., Psychology & Psychoanalysis. Associate Psychologist, The Roosevelt Hospital, New York, N. Y.

Bagley, Charles, III, M.D., Psychiatry-Adolescence. Associate in Psychiatry, University of Maryland Medical School, Baltimore, Md.

Ball, Erna D., M.D., Neurology & Psychiatry. Private Practice, Jamaica, N. Y.

Balter, Mitchell B., M.A., Social Psychology. Instructor, Psychology, University of Connecticut, Waterbury, Conn.

Barber, Radivoj Ray, M.D., Internal Medicine & Cardiology. Private Practice, Plymouth, Mich.

Barnett, Gordon J., Ph.D., Clinical Psychology-Psychoanalysis. Private Practice, Wantagh, L. I., N. Y.

Barry, John Reagan, Ph.D., Personality & Aptitude Assessment. Associate Professor, Psychology, University of Pittsburgh, Pittsburgh, Pa.

Barski, Georges, Sc.D., Virology. Chief of Research, French National Council for Scientific Research, Hôpital Saint Antoine, Paris, France.

Barton, David J., M.D., Biology. Senior Surgeon, Malden Hospital, Malden, Mass.

Batchelder, Walter E., M.D., Public Health. Medical Director, Cancer Commission, California Medical Association, San Francisco, Calif.

Becher, Harold, B.S., Chemistry. Consulting Engineer, Brooklyn, N. Y.

Beckert, William H., M.S., Cytology. Instructor, Biology, West Hempstead, N. Y.

Bedell, George N., M.D., Clinical Pulmonary Physiology. Assistant Professor, Internal Medicine, State University of Iowa College of Medicine, Iowa City, Iowa.

Bell, Samuel D., Jr., M.D., Microbiology. Assistant Professor, Harvard University School of Public Health, Boston, Mass.

Bellis, Edward D., Ph.D., Ecology. Assistant Professor, Zoology, Pennsylvania State University, University Park, Pa.

Berkowitz, Donald, M.D., Internal Medicine. Assistant Chief, Philadelphia General Hospital, Philadelphia, Pa.

Bigelman, Leo, M.D., Psychiatry. Private Practice, Hollywood, Calif.

Blades, H. W., B.S., Biology & Chemistry. President, Wyeth Laboratories, Philadelphia, Pa.

Bliss, Harry A., M.D., Internal Medicine. Assistant Professor, University of Illinois College of Medicine, Chicago, Ill.

Bogacz, John, Ph.D., Biology. Research Assistant, Hahnemann Medical College, Philadelphia, Pa.

Bolin, Vern S., M.S., Tissue Culture. Research Associate, Wistar Institute, Philadelphia, Pa.

Bouvier, Claude A. B., M.D., Medical Biology. Research Fellow, Cornell University Medical College-New York Hospital, New York, N. Y.

Bowen, Willet R., M.D., Pediatric Biochemistry. Instructor, Pediatrics, Syracuse University College of Medicine, Syracuse, N. Y.

Boyden, Douglas Gregg, M.D., Medicine. Lieutenant, U. S. Naval Submarine Base, New London, Conn.

Bradley, David John, M.D., Pediatric Cardiology. Private Practice, Cincinnati, Ohio.

Bradley, William F., M.D., Medicine. Private Practice, Internal Medicine, Columbus, Ohio.

Brandt, Ira Kive, M.D., Pediatric Metabolism. Assistant Professor, Pediatrics, Yale University School of Medicine, New Haven, Conn.

Brockman, Hiram LeRoy, Jr., M.D., Surgical Research. Chief, Experimental Surgery, M.D. Anderson Hospital and Tumor Institute, Houston, Texas.

Browne, Harry Gray, M.D., Biology. Resident in Pathology, The New York Hospital, New York, N. Y.

Brownell, Leona Weiss, M.S., Endocrinology. Research Assistant, Boston University, Boston, Mass.

Burtch, Robert B., M.D., Medicine. Private Practice, Alexandria Bay, N. Y.

Campanella, Joseph Thomas Carmine, B.S., Basic Sciences & Psychology. Instructor at C. I. C., Secaucus, N. J.

Cesarman, Eduardo, M.D., Pathology. Resident, Pathology, The New York Hospital, New York, N. Y.

Chriss, John W., M.D., Internal Medicine. Physician, Private Practice, Corpus Christi, Texas.

Clifford, Robert H., M.D., Plastic & Reconstructive Surgery. Chief, Plastic Surgery, Henry Ford Hospital, Detroit, Mich.

Cochran, Burt, Jr., M.D., Medicine & Biology. Assistant Clinical Professor, College of Medical Evangelists, Los Angeles, Calif.

Coleman, Lester Laudy, M.D., Medicine. Associate Attending Surgeon, Manhattan Ear and Throat Hospital, New York, N. Y.

Collins, Robert Douglas, B.A., Internal Medicine. Assistant Research Hematologist, New York Medical College, New York, N. Y.

Conner, S. Kent, M.D., Internal Medicine. Intern, Lois Grunow Memorial Clinic, Phoenix, Ariz.

Connor, William E., M.D., Atherosclerosis, Blood Coagulation. Research Fellow, State University of Iowa, Iowa City, Iowa.

Cornwell, David George, Ph.D., Biochemistry. Assistant Professor, Ohio State University, Columbus, Ohio.

Corylios, Elizabeth V.D., M.D., General Surgery. Senior Resident in General Surgery, Bellevue Hospital, New York, N. Y.

Crawley, L. Q., M.D., Biology. Private Practice, New York, N. Y.

Crelin, Edmund S., Ph.D., Structure & Physiology of Connective Tissue. Assistant Professor, Anatomy, Yale University School of Medicine, New Haven, Conn.

Da Cal, Ernesto Guerra, Ph.D., Romance Linquistics. Professor, Spanish & Portuquese, New York University, New York, N. Y.

Dao, Thomas L., M.D., Endocrinology. Chief, Department of Breast Surgery, Roswell Park Memorial Institute, Buffalo, N. Y.

Davis, Robert J., M.D., Medicine. Pathologist, St. Joseph's Hospital, Tampa, Fla.

de Girolamo, Antonio, M.D., Anatomy. Chief, Department of Anatomy, Naples Veterinary School, Naples, Italy.

de Groot, Jack, M.D., Neuroendocrinology. Assistant Professor, Physiology, Baylor University College of Medicine, Houston, Texas.

Douros, John D., Jr., Ph.D., Microbiology. Associate Microbiologist, Parke, Davis and Company, Detroit, Mich.

Dowling, LeNeal E., B.S., Biochemistry. Biochemist, Bedford Veterans Administration Hospital, Bedford, Mass.

Downing, Joseph J., M.D., Social Psychiatry. Director, New York State Department of Mental Health, Syracuse, N. Y.

Dröse, J. H., Ph.D., Microchemistry. Pharmacist, Bomi Hills Hospital, Monrovia, Liberia.

Dunkin, Morton L., M.D., Obstetrics. General Practitioner, Albuquerque, N. Mex.

Ecker, Herbert A., M.D., Medicine. Chief, Plastic Surgery, Williamsport Hospital, Williamsport, Pa.

Enson, Yale, M.D., Internal Medicine. Resident, Internal Medicine, Kings County Hospital, Brooklyn, N. Y.

Epstein, Marvin A., M.D., Internal Medicine. Private Practice, Walnut Creek, Calif.

Everitt, Neill J., M.D., Medicine. Private Practice, Omaha, Nebr.

Exner, Frederick B., M.D., Radiology. Physician, Private Practice, Seattle, Wash.

Fahlberg, Willson J., Ph.D., Immunology. Assistant Professor, Microbiology, Baylor University College of Medicine, Houston, Texas.

Fainstat, Theodore, M.D., Obstetrics & Gynecology. Research Fellow, Reproductive Physiology, Harvard University, Cambridge, Mass.

Fersko, Perry M., M.D., Metabolic Disease & Hematology. Assistant Resident, Department of Internal Medicine, Brooklyn Veterans Administration Hospital, Brooklyn, N. Y.

Fink, Mary Alexander, Ph.D., Medical Bacteriology. Assistant Professor, Microbiology, University of Colorado School of Medicine, Denver, Colo.

Fink, Max, M.D., Experimental Psychiatry. Director, Department of Experimental Psychiatry, Hillside Hospital, Glen Oaks, N. Y.

First, Arthur, M.D., Gynecology & Obstetrics. Associate Professor, Jefferson Medical College, Philadelphia, Pa.

Fisher, Hans, Ph.D., Nutrition and Biochemistry. Associate Professor, Nutrition, Rutgers University, New Brunswick, N. J.

Flanagan, Thomas L., B.S., Biochemistry. Group Leader, Philadelphia, Pa.

Fletcher, Thomas F., M.D., Pediatric Cardiology. Associate in Pediatrics, The Foss Clinic, Danville, Pa.

Fomon, John J., M.D., Surgery. Assistant Professor, University of Miami School of Medicine, Miami, Fla.

Fong, Willis M., M.D., Medicine, Pediatrics. Physician, Private Practice, Seattle, Wash.

Forestelle, W. H., M.D., Medicine. Physician, Private Practice, Seaside, Calif.

Fox, Allen S., Ph.D., Biochemical and Immunological Genetics. Associate Professor, Zoology, Michigan State University, East Lansing, Mich.

Frank, Steven A., A.B., Obstetrics & Gynecology. Medical Student, New York University-Bellevue Medical Center, New York, N. Y.

Freed, Herbert, M.D., Psychoanalysis. Professor, Clinical Psychiatry, Temple University Medical School, Philadelphia, Pa.

Fried, John, Ph.D., Synthetic Organic. Research Chemist, Merck and Company, Rahway, N. J.

Friedberg, Simeon A., D.Sc., Physics of Solids. Associate Professor, Carnegie Institute of Technology, Pittsburgh, Pa.

Friedlander, Emil, M.D., Dermatology & Syphilology. Instructor, University of Cincinnati Medical School, Cincinnati, Ohio.

Froede, Richard C., M.D., Pathology. Resident, Pathology, Letterman Army Hospital, San Francisco, Calif.

Fry, E. Lee, M.D., Medicine. Private Practice, Macon, Ga.

Fuchs, Frieda W., M.D., Chemistry, Radiology. Research Associate, Institut Fuer Chemische Technologie, Technische Hochschule, Aachen, Germany.

Fukuda, Tetsuo, M.D., Neurobiology. Research Associate, University of Pennsylvania Graduate School of Medicine, Philadelphia, Pa.

Fukutake, Katsuhiro, M.D., Blood Coagulation. Research Associate, Tufts University School of Medicine, Boston, Mass.

Gabriel, Karl L., V.M.D., Pharmacology & Toxicology. Assistant Director, Industrial Biology Research & Testing Laboratories, Philadelphia, Pa.

Gelormini, Otto, M.D., Internal Medicine. Practicing Internal Medicine, Syracuse, N. Y.

Gibson, Sam T., M.D., Blood Banking. Director, Blood Program, American National Red Cross, Washington, D. C.

Gold, Kenneth, M.S., Protozoology. Laboratory Assistant, Haskins Laboratories, New York, N. Y.

Gondos, Joseph, M.D., Medical Sciences. Retired Physician, Arlington, Va.

Gottfried, Bernard, M.D., Physiopathology of Cancer. Director, Clinical Research, Waldemar Medical Research Foundation, Port Washington, L. I., N. Y.

Graber, Edward A., M.D., Obstetrics & Gynecology. Practicing Physician, New York, N. Y.

Grady, John F., S.T.B., Psychology. Director, Catholic Chaplains Training Program, Columbus State Hospital, Columbus, Ohio.

Graham, Garth K., M.D., Biology. Internist, Private Practice, Los Angeles, Calif.

Graziano, Sophie, A.B., Biochemistry. Technician, State University of New York, Kings County Hospital, Brooklyn, N. Y.

Greenberg, Harold A., M.D., Psychiatry. Assistant Professor, University of Illinois, Chicago, Ill.

Grinvalsky, Henry T., M.D., Pathological Anatomy. Associate Pathologist, Mt. Sinai Hospital, Chicago, Ill.

Gwatkin, Ralph B., Ph.D., Microbiology. Research Associate, University of Toronto, Toronto, Ontario, Canada.

Ham, John P., M.D., Medicine. Local Health Officer, Yuma, Colo.

Hammon, William McD., M.D., Epidemiology. Professor, University of Pittsburgh, Pittsburgh, Pa.

Hammond, Keith, M.D., Pediatrics. Private Practice, Paoli, Ind.

Hamparian, Vincent V., Ph. D., Virology. Instructor, University of Pennsylvania, Philadelphia, Pa.

Hansen, Harold Norman, M.A., Psychology. Psychologist, Essex County Overbrook Hospital, Cedar Grove, N. J.

Harrell, George T. Jr., M.D., Metabolic Aspects of Infectious Diseases. Dean, College of Medicine, University of Florida, Gainesville, Fla.

Hartman, Lawton M., III, Ph.D., Physics & Linquistics. Private Practice, New York, N. Y.

Hašek, Milan, M.D., Transplantation Immunity. Director, Laboratory, Biologicky Inst. CSAV, Academy of Sciences, Prague, Czechoslovakia.

Haugen, Gerhard B., M.D., Psychiatry. Assistant Clinical Professor, University of Oregon Medical School, Portland, Oreg.

Herbert, Victor, M.D., Internal Medicine-Hematology. Senior Research Fellow, New York Heart Association, Montefiore Hospital, New York, N. Y.

Hervert, J. William, M.D., Internal Medicine, Instructor, Medicine, Creighton Medical College, Omaha, Nebr.

Hill, Robert Matteson, Ph.D., Electron Physics. Research Specialist, Microwave Physics Laboratory, Palo Alto, Calif.

Hilleman, Maurice R., Ph.D., Virus, Cancer. Director, Merck Institute for Therapeutic Research, West Point, Pa.

Hiramoto, Raymond, Ph.D., Immunology. Senior Cancer Research Scientist, Roswell Park Memorial Institute, Buffalo, N. Y.

Hoey, Richard, B.S., Veterinary Medicine. Veterinary Student, Ridley Park, Pa.

Holmes, Kendall B., M.D., Internal Medicine. Private Practice, Fresno, Calif.

Horton, Granville E., M.D., Nuclear Medicine. Practicing Physician, Wharton, Texas.

Householder, James R., M.D., Anesthesiology. Instructor, University of Washington School of Medicine, Seattle, Wash.

Howells, John D., Ph.D., Microbiology. Associate Research Microbiologist, Parke, Davis and Company, Detroit, Mich.

Hunter, Charles A., Jr., M.D., Physiology of Reproduction. Associate Professor, Obstetrics & Gynecology, University of Kansas Medical Center, Kansas City, Kans.

Iacono, Lawrence C., M.S., Research & Development. Organic Research Chemist, Rutherford, N. J.

Jack, Edgar A., M.D., Medicine. Senior Clinical Research Associate, Ciba Pharmaceutical Products Inc., Summit, N. J.

Jacobs, Herbert M., M.D., General Surgery. Physician, New York, N. Y.

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NOTICE TO ACTIVE MEMBERS

In accordance with the Bylaws of the Academy, Article VII, Section 7, the Nominating Committee of the Council is inserting in this April issue a notice to Active Members, quoting Article IV, Section 1 of the Bylaws. Article IV, Section 1 reads as follows:

"*Suggestions by the Members for Nominations.* Suggestions for nominations of Officers of the Academy and Members of the Council, with the exception of Sectional Chairmen, may be sent in writing by any Active Member of the Academy addressed to the Nominating Committee of the Academy, 2 East Sixty-third Street, New York, N. Y., with the name of the proposer. To be considered, such suggestions must be received not later than September 1 of each year."